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ARTICLES

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THE STATE IN POLITICAL SCIENCE: HOW WE BECOME WHAT WE STUDY

THEODORE J. LOWI *Cornell University*

American political science is a product of the American state. There are political reasons why particular subdisciplines became hegemonic with the emergence of the "Second Republic" after World War II. The three hegemonic subdisciplines of our time are public opinion, public policy, and public choice. Each is a case study of consonance with the thought-ways and methods of a modern bureaucratized government committed to scientific decision making. Following Leviathan too closely results in three principal consequences: (1) failure to catch and evaluate the replacement of law by economics as the language of the state, (2) the loss of passion in political science discourse, and (3) the failure of political science to appreciate the significance of ideological sea changes accompanying regime changes.

This presidential pilgrimage is over, and I can report that the American Political Science Association is alive and well. But a pilgrimage is not a journey into happiness. A pilgrimage is a search, and no pilgrimage is fulfilled until the the pilgrim returns and shares the pains of discovery.

From out of their early pilgrimage, the Quakers cried, "Speak truth to power." From out of my pilgrimage I responded, "Who's listening?" and "What truths do we have to impart?" On my pilgrimage I listened in on the conversation between political science and power, and it is my duty to report that the terms of discourse have been set by power. We are not the teachers we thought ourselves to be.

The insights of my pilgrimage began with my awakening to three facts: (1) U.S. political science is itself a political phenomenon and, as such, is a product of the state; (2) there is not one science of politics but several, each the outcome of a particular adaptation to what it studies; and (3) even assuming that we are all sincerely searching for the truth (and it is more interesting to assume that), there are reasons other than the search for truth why we do the kinds of political science we do and why particular subdisciplines become hegemonic. In sum, every regime tends to produce a politics consonant with itself; therefore every regime tends to produce a political science consonant with itself. Consonance between the state and political science is a problem worthy of the attention of every political scientist.

To explore the relation between the state and political science, I have chosen case studies of the three hegemonic subdisciplines of our time—public opinion, public policy, and public choice—preceded by an overview of the transformation from the old to the new state and the old to the new political science. I will conclude with a brief evaluation of the consequences for political science of being a "dependent variable."

There is no need to document for political scientists the contention that the American state until the 1930s was virtually an oxymoron. The level of national government activity was almost as low in 1932 as it had been in 1832. However, although a number of large social movements had failed to expand the national government after the Civil War, they had succeeded in nationalizing the focus of U.S. politics. The Civil War and industrialization made us one nation in fact. *Wabash, St. Louis, and Pacific Railway* v. *Illinois* of 1886 (118 U.S. 557) contributed with the doctrine that the state governments were constitutionally incompetent to confront the nationalizing economy. The media transferred their dependence from the highly localized political parties to the corporations seeking mass sales through advertising.

Political science as a profession was a product of this nationalization of political focus. Intellectual historians such as Somit and Tannenhaus (1967) and Seidelman (1985) report that the APSA was part of the progressive reform movement. Somit/Tannenhaus report that only 20% of the first decade's membership were "professors and teachers" (p. 55). From out of the beginnings in the 1890s, where the writing was "legalistic, formalistic, conceptually barren and largely devoid of what would today be called empirical data" (p. 69), the founders of the association were committed to political realism, which meant facts, the here and now, and the exposure of the gap between the formal institutions and the realities. James Bryce in his address as the fourth association president in 1909, urged political scientists to "Keep close to the facts. Never lose yourself in abstractions. . . . The Fact is the first thing. Make sure of it. Get it perfectly clear. Polish it till it shines and sparkles like a gem" (quoted in Somit and Tannenhaus 1967, 70). The title of Woodrow Wilson's presidential address to the seventh annual meeting of the APSA was "The Law and the Facts." Early in his speech he said, "I take the science of politics to be the accurate and detailed observation of [the] processes by which the lessons of experience are brought into the field of consciousness, transmuted into active purposes, put under the scrutiny of discussion, sifted, and at last given determinate form in law" (1911, 2). But these were not

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facts for themselves alone. Some early political scientists were active reformers, others were radical muckrakers, and a few may have been completely aloof. But facts were to be put in the service of assessment: Did a given political institution meet its purpose? According to Wilson, political scientists should serve as a kind of "self-constituted commission . . . to discover, amidst our present economic chaos, a common interest, so that we might legislate for the whole country instead of this, that, or the other interest, one by one" (pp. 6–7).

There is no evidence to suggest that the founding generation were trying to form an *intelligentsia*, defined as an organization of intellectuals in opposition to the state. There was, in fact, no state to organize against. If anything, there was a memory trace of the two states that conducted the most devastating total war in history up to 1865. But both states were dismantled quickly after the Civil War and were folded back into the "stateless polity" of the restored Union (Bensel 1990). One could say, however, that the early APSA was a kind of *counter*intelligentsia formed in defense of a state that did not yet exist. The political science of the entire first generation of the APSA was formed around politics—the observable, the immediate, and the short-run purpose to be served. But politics was not only a phenomenon, it was a problem. For example, to Goodnow, the purpose of the political science was to show "particularly from a consideration of political conditions as they now exist in the U.S., that the formal governmental system as set forth in the law is not always the same as the actual system" (quoted in Ross 1991, 274). And for most of them, there was a handy solution to the problem of politics—government, properly characterized as the "building of a new American state" (Skowronek 1982). This goal of a new American state can, in turn, be characterized as a stateless government, or an enlightened administration. Woodrow Wilson, while still an obscure professor of political science at Johns Hopkins, sounded the call for the study of administration in 1887. This should be understood, however, within the context of his still larger declaration that the era of constitution making was closed "so far as the establishment of essential principles is concerned" (quoted in Ross 1991, 275). Administration could be a solution to politics because, in Wilson's words, we could have the Prussian state breathe free American air (Wilson 1887). As Seidelman puts it, "the study of politics for Wilson thus had to evolve into a study of America's cultural uniqueness and European administration" (1985, 44). Wilson was confirming the unarticulated major premise of political science, namely, that the American system was permanent and that the science of politics involved the study and assessment of political things within a permanent and unique context. We were one republic, then and forever. Political scientists could remain a counterintelligentsia not because all members shared the Lockean liberal consensus but because they were scientists in the state-building business even while, as with Bentley, they were attacking the very concept of the state as "soul stuff" (ibid., 70–71). For the same reason, political science was atheoretical. Works produced by the founding generation stand up well even by today's standards of science and are superior to most of ours in the quality of the knowledge they brought to bear and in their use of the English language. But the work remained essentially empirical and became almost technocratic in its participation in the reform movement, primarily because it had no concept of an alternative regime in the United States.

It should have been unmistakably clear to any political scientist of 1887 or later that the American system after the Civil War was a new regime, deserving a new name. Why not the Second Republic? The answer is that that would have suggested an impermanence to the American regime. If a Second Republic, why not a Third and Fourth? My wife sometimes introduces me to her friends as her first husband. That is a sobering sobriquet. Political science was atheoretical because it had no concept of a Second Republic or of any other alternative regime. Eventually, political scientists would virtually rewrite democratic theory to accommodate political parties and would rewrite republican theory to accommodate the devolution of constitutional powers from Congress to the presidency. But this was not a self-conscious act of political theory; it was part of "the study of political conditions as they exist." In the stateless polity of the founding epoch, the science of politics was the study of politics and of political institutions within a timeless, as well as a uniquely American, framework.

In my opinion, the golden age of U.S. political science came toward the end of this founding epoch, which corresponds, of course, with the end of the stateless polity. Works of political science of the 1930s and 1940s were magnificent in their ability to describe a complex political whole; thorough, honest, and imaginative in their use of statistics to describe a dynamic reality; and powerful and cogent in pointing out flaws and departures from U.S. ideals. But this was the sentimental part of my journey. To yearn for those particular studies of elections, case studies of interest groups and policymaking, histories of party systems, and representation in Congress is to yearn also for the luxury of the First Republic, now that we are irreversibly in the Second and possess at least the bare beginnings of an awareness of the possibility of regime change in the United States.

Surely by now there has been, in fact, a change of regime, which I call the Second Republic, for lack of an established enumeration. It is not the French state or the Prussian state; but at least, we can say that the American state is no longer an oxymoron. Here, all too briefly, are its relevant high spots: (1) it is a positive, not a reactive, state, from the start centered on the executive branch; (2) constitutional limits on the powers of the national government over the economy and on the distribution of power among the branches within the national government were very quickly laid to rest; (3) many aspects of politics that had traditionally been private (e.g., registration, bal-

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lots, election administration, nomination, job patronage, polling, and campaign finance) have been governmentalized—that is, modern government has assumed responsibility for its own politics; (4) political parties, like nuclear families, have declined for lack of enough to do; (5) bureaucracy, independent of party and Congress, has expanded in size and scale approaching autonomy as a social force; and, (6) intimately connected with (5), government has become intensely committed to science. This was no accident, and it is no mere policy. Science is an inherent part of the new, bureaucratized state, in at least two dimensions: First, it involves a commitment to building science as an institution, that is, a commitment to government *for* science; and second, it involves a commitment to government *by* science—that is to say, it involves scientific decision making. This has been properly characterized as *technocratization*, which I take to mean "to predict in order to control" (compare Mills 1959, 113). But another to-me-more-interesting but less appreciated part of this aspect of the expansion of science is that *economics has replaced law as the language of the state.*

What Tocqueville said of the First Republic we may say of the Second: "A new science of politics is needed for a new world" (quoted in Wood 1969, v). But life is not quite so simple. If modern states are differentiated, there are almost certain to be several sciences of politics, rather than just one. We tend to call these subdisciplines; but despite continuities and overlaps, they are quite distinct. Each can be understood as a product of the phenomena it studies; but I am concerned here not to explain or place them all but only to understand the "hegemonics" of disciplines—why public opinion, public policy, and public choice became hot topics and when.

Some call public opinion behavioral science. I think I am more accurate calling it public opinion. Observers from an alien intellectual planet would find it most peculiar that the study of individual opinions and attitudes could be called behavioral—until they deconstructed the discourse between the new bureaucratized state and the new political science. Here is my deconstruction:

1. If science is to be public, it must be neutral.

2. It must also be rational and therefore concern itself with rational phenomena, that is, orderly, repeatable, predictable phenomena. This is precisely what makes science and bureaucracy so compatible. Karl Mannheim, in 1929, over 20 years before the behavioral revolution, wrote, "Bureaucratic thought is permeated by measurement, formalization, and systematization on the basis of fixed axioms . . . [such that] only those forms of knowledge were legitimate which touched and appealed to what is common to all human beings" (1936, 167).

3. Science also has to be microscopic, down to the irreducibly smallest unit. It is no paradox that as our state grew larger, the units of analysis in our social science became smaller. This is a profoundly important aspect of rationality: out of small units, large numbers grow; and large numbers behave according to the regularities of mathematical probability. (In this context it is easy to understand why Arthur Bentley's appeal "to fashion a tool" with the group as the smallest unit of analysis was first uttered in 1907 and not really heard, or responded to, until over 40 years later [see Seidelman 1985, 72–74].)

4. Science, like administration, has to follow a prescribed method. As Robert Wiebe put it, "Bureaucratic thought . . . made 'science' practically synonymous with 'scientific method'. Science had become a procedure . . . rather than a body of results" (1967, 147).

5. The language itself has to be microscopic; that is, science has to be translated into the language of variables.

The phenomena and methodology of public opinion obviously meet all the requirements of a science that would be consonant with bureaucratic thinking. And now consider the units of analysis within the sample surveys that give public opinion its link to political behavior: voting and participation. These display an even stronger consonance with the state, in that these are approved political behavior (i.e., political behavior sponsored by the state and needed by regimes and elites to maintain their legitimacy).

Some see behavioral science as a large step toward hard science and, through that, an advancement toward greater enlightenment about society and politics. I do not disagree. But my political analysis tells me also that the hegemony of the subdiscipline of behavioral science or public opinion was to a large extent a product of its compatibility with bureaucratic thought-ways, rather than the result of successful discourse within political science.

It is important to emphasize, however, that the hegemony of the subdiscipline of public opinion is a case of natural selection, not one of political maneuvering or intellectual opportunism. Anyone personally acquainted with the people who made the behavioral revolution in political science would agree that if political skill were required to succeed, there would be no survey research centers—probably no behavioral science at all. It is their very lack of attention to playing the political game that makes the success of their field so interesting. The explanation is to be found not in politics in the vulgar sense but politics in the higher sense—the politics of state building.

The Second Republic, having put a new emphasis on science, also determined what that science would be. The capacity to engage in public opinion research in political science had been in existence since at least the late nineteenth century. Statistics, which takes its name from *state* and *statist*, reached maturity still earlier in the nineteenth century and grew in importance as states democratized and individuals began to "count" for something. Sampling was also well advanced and widely practiced, especially in the agricultural sciences (Porter 1986, 23–25). Even opinion polling in political campaigns was actually tried at least as early as 1892, albeit over the objections of many defenders of the sanctity of elections (Jensen 1969, 228–229)—and was picked up by advertising

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companies and newspapers soon after. Yet public opinion did not become the hegemonic subdiscipline of political science until the Second Republic.

Public policy as a subdiscipline of political science has an even longer genealogy than public opinion, although it was more than a decade later in emerging as a hegemonic subdiscipline. The study of public policy begins, of course, with the study of legislation, whose history is usually traced out from divine law through common law to something called positive law, to indicate the demystification of law and the deliberateness of modern laws. There is, then, one later stage called public policy, indicating the intervention of administration between legislature and citizen. Public policy is a term of art reflecting the interpenetration of liberal government and society, suggesting greater flexibility and reciprocity than such unilateralist synonyms as *law, statute, ordinance, edict,* and so on. Public policy began to gain some currency in public administration in the 1930s; and public administration had been one of the hegemonic subdisciplines in the political science of the stateless polity I refer to as the First Republic. The decline and transfiguration of public administration gives us the key to public policy. Traditional public administration was almost driven out of the APSA by the work of a single, diabolical mind, that of Herbert Simon. Simon transformed the field by lowering the discourse. He reduced the bureaucratic phenomenon to the smallest possible unit, the decision, and introduced rationality to tie decisions to a system—not to any system but to an economic system. His doctorate was in political science; his Nobel award was in economics.

Now, Simon did not accomplish this all by himself. His intellectual tour de force was made possible by actual changes in the administrative institutions of the Second Republic. Administrative authority in the First Republic partook of a fairly well established tradition of separating public from private life by a variety of legal rules and procedures that comprise what Joseph Vining calls the "masterful myth of the 'rule of law' "; in the Second Republic, these rules and myths broke down—not spontaneously, but in face of the rise of economic thinking in the corporate world, as well as in government (1978, 27).

It is in this context that modern public policy became a hegemonic subdiscipline in political science, overshadowing behavioralism itself. The study of public policy in the political science of the First Republic drew upon public law and institutional economics. Some of that old-fashioned public policy study exists today. But the modern approach is more appropriately called public policy analysis, which draws upon macroeconomic methods and economic systems thinking. The best way to demonstrate the size and character of this new subdiscipline of political science is to point to the presence of the policy analysis courses within political science departments and the explosive growth of the separate policy analysis programs and the economics requirements in the schools of public affairs and public policy and in the law schools. All the students in those places are learning the new language of the state.

It does no disservice to the subdiscipline of public choice to tie it to another of Karl Mannheim's observations dating from 1929, namely, that in the political science of a bureaucratic state "an economic man, a political man, etc., irrespective of time and race, could be constructed on the basis of a few axiomatic characteristics" (1936, 167–68). Mannheim continues: "Only what could be known by the application of these axioms was considered as knowable. Everything else was due to the perverse 'manifoldness of the real', concerning which 'pure' theory need not worry itself" (p. 168). Compare this to Kenneth Arrow's assertion made in a boastful spirit nearly 40 years later that any assumption other than the rational actor leads to mysticism and irrationality (Goldfield and Gilbert 1990, 14–15). This gives us a start toward a political explanation for why public choice has become probably the hottest thing going in political science today.

Quite aside from whatever merits it may have as a method and however true its truths may be, public choice is hegemonic today for political reasons or (to be more dignified about it) for reasons of state. Let me dramatize this in quite tangible rational actor terms: most of the luminaries in this subfield of political science came from, serve in, or are substantially associated with the same freshwater universities that kept burning the flame of laissez-faire ideology: Chicago, Rochester, Washington University of Saint Louis (nor should we overlook the Saint Louis Federal Reserve staff) (Johnson 1991). Here, again, we are confronting not political opportunism but institutional consonance—a symbiotic relationship between state and political science.

The affinity between modern bureaucratic government and economics—already strong—was further strengthened by the revival of the political popularity of laissez-faire ideology within the Republican party. For most of this century, laissez-faire liberalism (erroneously called conservative) had been the Republican party's center of gravity; but after the Depression, it had had little effect on voters and even less on intellectuals in the social sciences. Few intellectuals figured in national Republican party circles. *Conservative intellectual* was just another oxymoron. Today, of course, Republican administrations are overflowing with intellectuals, as are affiliated think tanks and the op-ed pages of the major newspapers. I see no signs yet of a Republican takeover of the APSA; but I do see one beneficiary of the Republican party era in political science, namely, public choice. People of merit inhabit this subfield, but its hegemony has little to do with their merit. Their success as a group was entirely fortuitous.

We political scientists enjoy the primitive wisdom of Mr. Dooley; and probably Mr. Dooley's best-known scientific proposition was, "No matter whether th' constitution follows th' flag or not, th' supreme court follows th' iliction returns." A more dignified Dooley would say, "The APSA follows

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Leviathan." I conclude with what are to me the three principal consequences of following Leviathan too closely. First, we have as a consequence failed to catch and evaluate the significance of the coming of economics as the language of the state. Second, we have failed to appreciate how this language made *us* a dismal science like economics. Third, having been so close to Leviathan, we failed to catch, characterize, and evaluate the great ideological sea changes accompanying the changes of regime.

First, then, why economics? Of what use is economic analysis to politics? Since economics was always a deeply flawed predictive science, why was it so attractive to policymakers and bureaucrats in the new state? Why was it so attractive to political science? My evaluation was inspired in part by an observation made 30 years ago by the distinguished economic philosopher Joan Robinson: "Economics . . . has always been partly a vehicle for the ruling ideology of each period as well as partly a method of scientific investigation" (1962, 1). My answer is that economic analysis is politically useful because it closes off debate, especially in a highly public representative assembly like Congress. The rise of economics as the language of the state parallels the decline of Congress as a creative legislature. (I argue this point more extensively in Lowi 1991.) Policymaking powers are delegated less to the agency and more to the decisionmaking formulas residing in the agency. The use of economic analysis to close off debate was strengthened as Republicans discovered that economic analysis could be used as effectively for them as for the Democrats—by manipulating the cost, rather than the benefit, side of cost–benefit analysis. I recommend John Schwarz's evaluation of Murray Weidenbaum's outrageous manipulation of the "costs of regulation" that supported the Reagan administration's commitment to deregulation (1988, 90–99). But I must confess that both the Democratic and the Republican politicians were smarter than the political scientists, because they took the stuff as weaponry, while we took it as science. We swallowed economics before subjecting it to a political analysis.

We should have seen that economics rarely even pretends to speak truth to power. If substantive truths were claimed, there would be room for argument. But economics, particularly as a policy science, stresses method above all. And the key to the method is the vocabulary of economics, which is the *index*. An index is not a truth but an agreement or convention among its users about what will be the next best thing to truth. M_1, the Dow, the CPI, unemployment, GNP: This is the new representative government—an index representing a truth. Indices have analytic power because they fit into defined systems; and of course, systems are also not truths but only useful fictions. (This, by the way, is not an attack on indices *or* systems. It is just a political evaluation of indices and systems.)

Now to the second of my consequences, that is, that the modern bureaucratic state has made political science just another dismal science. By *dismal* I do not mean merely the making of gloomy forecasts, in the Malthusian tradition: I mean the absence of *passion*.

During my pilgrimage, the most frequent complaints I heard were against the *American Political Science Review*. I join in at least one of these, which I do not limit to the *APSR*. Too few of the articles seek to transcend their analysis to join a more inclusive level of discourse. There is consequently little substantive controversy. The response is that a scientific journal must be dedicated to replication and disproof. But actually, very few pieces independently replicate anything; and even if they did, replication alone is dismal stuff. Political science is a harder science than the so-called hard sciences because we confront an *un*natural universe that requires judgment and evaluation. Without this, there can be no love of subject, only vocational commitment to method and process. The modern state has made us a dismal science, and we have made it worse by the scientific practice of removing ourselves two or three levels away from sensory experience. Political scientists have always quantified whatever and whenever they could, and most tried to be rigorous; but they stayed close to sensory experience. Even with our original mechanical helper, the counter–sorter device, it was possible to maintain a sensory relation to the data. What a pleasure it was to watch the cards seek their slots! And what a pity today that the empiricists have only their printout!

Finally, I turn to our failure to catch or evaluate adequately the ideological sea changes accompanying the changes of regime. Time permits only the barest inventory of missed opportunities, but I think they will speak for themselves.

The perspective of nearly 50 years makes it easy to see what we did not catch about the New Deal as a regime change. Although political scientists caught the new liberalism in the air, they failed to evaluate whether all the elements of this ideology were consistent with liberalism or with constitutionalism. They failed, for example, to capture and evaluate the significance of "administrative law." They noticed, but merely celebrated, the delegation of power from Congress to the executive branch. At the time it meant only the fulfillment of the New Deal program. Even as time passed, our tendency was to render each change consistent with our existing model of the political system. There was virtually no serious political science inquiry into whether the changes in constitutional doctrine, governmental structure, and policy commitments constituted a regime change. Some Republicans suggested that the United States had become a socialist regime, but political science did not respond to this challenging formulation. It should be a matter of ultimate interest, as well as enjoyment, to fight intensely over the identification of criteria for determining when a political change is sufficient to constitute a regime change. The New Deal helped give us a new political science but did not provide sufficient inclination to evaluate what was new.

We are at this moment in the presence of another

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failure, namely, a failure to catch the nature and significance of the ideological shift accompanying the current Republican era. The inability of the Reagan administration to terminate any important New Deal programs should at least have led to a reflection on the nature of the New Deal as a regime change. Even a post hoc evaluation would be useful. Meanwhile, the Republican era has brought with it some profound ideological changes that political science is failing to capture even though our own public opinion polls are sensing them. Political science has failed to catch and evaluate the two separate components of the Republican coalition: the old, laissez-faire liberalism and the genuine, native conservatism. Political science has stood by and permitted Republican candidates and staff intellectuals to treat the traditional laissez-faire core of the Republican party as conservative and then to compound the felony by stigmatizing liberalism as an alien belief system akin to socialism. This profound misuse of rich terminology is literally poisoning political discourse in the United States, and political science has to take a lot of the blame for this. We also did not catch the rise of the genuine conservatism; although our polls were picking up significant reactionary movements, we continued to treat the Falwell phenomenon and such predecessors as the Christian Anti-communist Crusade as aberrant. And we have passively witnessed the joining of laissez-faire liberalism with genuine right-wing conservatism as though they are consistent in their opposition to big government. Laissez-faire liberal Republicans, supported by their economists, embrace an ideal of radical individualism and view all government as a threat to freedom. In contrast, genuine conservatives are not individualists but statists. The state they want consists of tight and restrictive police control by state and local governments, but they are statists nevertheless. Genuine conservatives were never really at home with purely market relations; and they have never espoused the ideal (much less the methodology) of rational individualism. (Many conservative Catholic lay intellectuals have tried in vain to establish a comfortable concordance with free market liberalism.) Conservative intellectuals are now writing the poetry of executive power and are the authors of most of the writing that bashes Congress and the politics of representative government. Just as political scientists did not catch the ideological significance of the propresidential power writings of the New Deal supporters in the 1950s and early 1960s, we are now not catching the significance of the fact that most of the current propresidential power writing is by the Far Right. The far right-wing intellectuals are also writing a significant proportion of the new work on the founding intended not only to contribute to historical scholarship but to reconstitute the constitution in such a way as to place the presidency above the law and affirmative action beneath it.

No effort has been made to camouflage my antagonism to Republican era ideology. But my own personal position is irrelevant. Political scientists of the Left, Right, and center are a unity in their failure to maintain a clear and critical consciousness of political consciousness. Causal and formal analyses of the relations among clusters of variables just will not suffice. Nor will meticulous analysis of original intent. It is time we became intellectuals.

At the end of my pilgrimage, I have come to the conclusion that among the sins of omission of modern political science, the greatest of all has been the omission of passion. There are no qualifications for membership in the APSA; but if I had the power to establish such standards, they would be that a member should love politics, love a good constitution, take joy in exploring the relation between the two, and be prepared to lose some domestic and even some foreign policy battles to keep alive a positive relation between the two. I do not speak for the passion of ideology, though I do not count it out. I speak for the pleasure of finding a pattern, the inspiration of a well-rounded argument, the satisfaction in having made a good guess about what makes democracy work and a good stab at improving the prospect of rationality in human behavior.

Regime changes throughout the world since 1989 ought to give us a clearer perspective on some new sciences of politics. Although only a few of the world's regime changes will be liberal democracies, they are stimulating tremendous demand for transferrable insights about the workings of liberal democratic institutions, especially U.S. institutions. May this demand draw U.S. political scientists out of the shadow of Leviathan upward and outward toward a level of discourse worthy of the problem. This is not an opportunity to play philosopher–king. It is an opportunity to meet our own intellectual needs while serving the public interest. And we need not worry how to speak truth to power. It is enough to speak truth to ourselves.

Note

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DEMOCRATIC THEORY AND SELF-TRANSFORMATION

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Democratic theories that argue for expanding the scope and domain of democracy assume that democratic experiences will transform individuals in democratic ways. Individuals are likely to become more public-spirited, tolerant, knowledgeable, and self-reflective than they would otherwise be. This assumption depends on viewing the self as socially and discursively constituted, a view that contrasts with the standard liberal-democratic view of the self as prepolitically constituted and narrowly self-interested. The importance of the social and discursive view of the self is that it highlights how standard assumptions about the self help to justify limits to democratic participation. As now conceptualized, however, the transformational assumption does not meet standard objections to expanding democracy. I sketch an approach that distinguishes classes of interests according to their potentials for democratic transformation, and strengthens—by qualifying—transformative expectations in democratic theory.

Within the discourse of democracy there is a pervasive split between two broad groups of theories. One group seeks to balance democratic participation against other desirable features of political order (e.g., protections of rights and freedoms, pluralism, and governability) by limiting the spheres of society that are organized democratically. The other group of theories sees such limits to democracy as an important cause for many of the ills of contemporary liberal democracies. As is well recognized, the disagreement stems in part from differing assumptions about the self and its relation to politics (Barber 1984, chap. 4; Bowles and Gintis, 1986, chap. 5; Cunningham 1987, 188–93; Dryzek 1990, 13–15; Gould 1988; Macpherson 1977; Mansbridge 1980, chap. 2; Mansbridge 1990a; Offe 1985, chap. 7; Pateman 1970). Theories that see trade-offs between democracy and other goods often hold that the self is defined by interests that are formed prepolitically, either reflecting fixed desires or formed by social institutions and other circumstances that are outside of institutionalized politics. Democracy, on this view, is primarily a means for aggregating prepolitical interests and should be limited in scope and domain just because it is instrumental to prepolitical interests and not a good in itself. I shall refer to these theories as *standard liberal democracy*.

In contrast, theories that argue for increasing the scope and domain of democracy (*expansive democracy*) hold that standard liberal democracy fails to articulate goods that are inherent in democracy and exaggerates the threats posed by democracy to other goods. On this view, these limitations follow from a more general failure of standard liberal democracy to appreciate the transformative impact of democracy on the self, a failure rooted in its view of the self as prepolitically constituted. On the expansive view, were individuals more broadly empowered, especially in the institutions that have most impact on their everyday lives (workplaces, schools, local governments, etc.), their experiences would have transformative effects: they would become more public-spirited, more tolerant, more knowledgeable, more attentive to the interests of others, and more probing of their own interests. These transformations would improve the workings of higher-level representative institutions, as well as mitigate—if not remove—the threats democracy is held to pose to rights, pluralism, and governability.

In making this argument, expansive democrats rely on three assumptions that run counter to standard liberal democracy. The first is that increased democracy transforms individualistic and conflicting interests into common and nonconflicting ones, in the process developing capacities of citizenship that reduce factional threats to rights and pluralism. Second, because these transformations reduce conflict, they allow reduced use of power as a medium of political interaction. This would increase consensus and governability, as well as being desirable in its own right. Third, far from being a threat to the dimensions of the self protected by rights and freedoms, democracy is necessary to the values of self-development, autonomy, and self-governance—the values that rights and freedoms presumably are designed to protect. Together, these assumptions make up what I shall refer to as the *self-transformation thesis*.

I shall examine the self-transformation thesis and the conceptions of the self that sustain it. My general argument is that when the thesis is properly identified, qualified, and refined, it is both defensible and essential to democratic theory. My support for the thesis is, however, critical. As now presented in the literature, it does not meet its burden of proof. More specially, I argue that the different kinds of attachments that constitute the self present different kinds of transformational problems. I sketch an approach that distinguishes classes of interests (expressions of these self-defining attachments) according to their potentials for transformation in nonconflicting ways and identifies transformations that are consistent with, or increase, the autonomy (capacity for self-direction) and development of the self. Thus, I identify spheres of interest within which expansive dem-

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ocratic theory ought to locate its transformative expectations, as well as spheres within which these expectations are undesirable. In the latter, democratic theory should recommend institutions designed to deal with conflict and based on decision rules that do not require transformations of the self.[1] I intend these refinements to address common objections to the self-transformation thesis, as well as to suggest new directions for expansive theories of democracy.

ASSUMPTIONS ABOUT THE SELF IN DEMOCRATIC THEORIES

With regard to assumptions about the self, democratic theory falls into two broad groups. *Standard liberal democracy* includes a number of otherwise diverse theories, including those of Madison and James Mill and the pluralist–elite theories of Weber and Schumpeter, as well as American pluralism and neoconservatism. While these theories differ in important respects, here I consider them together: they all assume that democracy is primarily of instrumental value for protecting or realizing the interests that belong to individuals or groups independently of political life. That is, standard liberal-democratic theorists conceive interests as prepolitical preferences. Political institutions aggregate preferences, but they do so without fundamentally changing them. Of course, these theories recognize as an empirical matter some transformative effects (as when leaders develop a sense of public responsibility or cooperative norms emerge between adversaries in Congress). But these recognitions are ad hoc and not theorized in ways that could help to identify transformative possibilities in other arenas of political life.

These characteristics of the standard view are reinforced by a more general assumption about the nature of politics. Politics, on this view, is an allocative or economic kind of activity, operating in a world of scarce values. For this reason, the paradigm of standard liberal democracy limits its conception of politics to scarcity-induced conflict. Its problems have to do with protecting individuals from excesses of political power, governing in the face of conflicting demands, and finding grounds for obligation to political order in prepolitical interests. Following from this way of problematizing politics, standard liberal democrats argue for institutional limits on democracy, seeing expansions of democracy as potential threats to other goods—especially freedom, pluralism, governability, informed decision making by political elites and experts, and efficient delivery of governmental goods and services.

Expansive democracy includes participatory democracy, democratic socialism, and the more radical strains of liberal democracy that stem from Rousseau, John Stuart Mill, T. H. Green, and John Dewey. These theories argue for increased participation in, and control over collective decision making, whether by means of direct democracy in small-scale settings, or through stronger linkages between citizens and institutions that operate on broader scales. They are often concerned with expanding democracy beyond traditional political spheres into economic and social spheres, that is, into arenas of power that are privatized in standard liberal democracy. Whereas, in the standard view, *democracy* most often refers to institutional processes such as competitive elections, in expansive theories it refers more broadly to the effects of institutions in increasing individuals' control over self-determination and self-development. This is why expansive theories recommend relating decision making to the persons who are affected whenever possible. Closely related, these theories assume that democracy has more than an instrumental value to ends such as freedom, protection of private satisfactions, security, and order. Democracy generates values that are intrinsic to political interaction and closely related to self-development. Limiting democracy to, say, electoral competition between elites (Schumpeter 1942, chap. 22) deprives nonelites of access to conditions of their own development.

Theories of expansive democracy should not be confused, as they often are, with communitarian critiques of liberalism (MacIntyre 1981; Sandel 1982; Sullivan 1982; Taylor 1979). Expansive democrats do share with communitarians an appreciation of the embodied and contingent nature of the self and a concern with alienation from public life. The differences lies in their approaches to reasserting connections between self and community. Communitarians often take what Seyla Benhabib calls an "integrationist" approach, leaving ambiguous the role of political empowerment in developing new communities (1989, 383). In contrast, expansive democrats insist on the centrality of democratic empowerment, dialogue, and interaction, holding that integration without democracy threatens to sacrifice self to community (Connolly 1991, chap. 3).

STANDARD OBJECTIONS TO EXPANDING DEMOCRACY

A list of standard liberal-democratic objections to expanding democracy will help identify the burden of argument that the self-transformation thesis must bear and will also identify key assumptions about the self in the standard view.

One common objection to expanding democracy stems from Hobbesian assumptions about political judgment that were absorbed into the Madisonian, utilitarian, and even Kantian traditions and more recently assimilated by rational choice theory (Mansbridge 1990a). On this view, individuals are rational maximizers of preferences, so their political judgments will follow self-interest in ways that are likely to threaten the rights and liberties of other individuals. For this reason, institutions should be designed to separate political judgments from those of individuals; democratic decision making should be repre-

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sentative, rather than direct. As Madison put it, "No man is allowed to be a judge in his own cause because his interest would certainly bias his judgment, and, not improbably, corrupt his integrity" (Madison, Hamilton, and Jay 1961, no. 10). Expansive democrats must show that democratic participation would mitigate this danger by developing individuals' appreciation of common interests, as well as their respect for the differing interests of others.

A second objection to expansive democracy follows from a related Hobbesian assumption, namely, that political interests are inherently conflicting because they are oriented toward scarce resources. On this view, politics is an allocative (or economic) activity that takes place within an environment of scarcity, an assumption incorporated into standard liberal and pluralist definitions of politics (e.g., Easton 1965, 50). For this reason, standard liberal democracy justifies institutions that are, in Jane Mansbridge's terms, essentially "adversarial" because it assumes, and is designed to deal with, interests that are conflicting. The limitation on democracy in this case is indirect: the general limitation on the sphere of political possibility rules out institutions that cultivate common goods through participatory interaction and dialogue (Elkin 1985; Macpherson 1977; Mansbridge 1980; Xenos 1987). To justify expansions of democracy in this case, expansive democrats need to show that not all activities of politics are allocative (or economic) in nature because not all politically relevant interests are oriented toward scarce goods.

Third, according to standard liberal democracy, even if resources were not scarce, it is undesirable to subject a plurality of individual ends to politics (as expansive democracy would), since doing so inevitably subjects individual wills to a general will, and this leads to coercive results (Nozick 1974, 310; Riker 1982, chap. 1; Sartori 1987, 315–20). Kant, for example, referred to democracy as "necessarily a *despotism*" because it "establishes an executive power through which all the citizens may make decisions about (and indeed against) the single individual without his consent" (1970, 101; emphasis original). Likewise, Madison feared that expanding political participation would threaten a tyranny of the majority against minorities. His solution, of course, was to limit democracy by limiting the ability of majorities to use the powers of government. Standard liberal democracy holds, in effect, that it is desirable to depoliticize as many spheres of society as possible, rather than to democratize them. Depoliticization also means limiting spheres of democracy and leaving social life to be ruled by nongovernmental powers. To deal with the possibility that more democracy will lead to majority tyranny, expansive democrats must show that democratic experiences develop individuals' respect for persons, pluralism, and freedom.

A fourth objection to expansive democracy within standard liberal democracy is most closely associated with American pluralism, and has been articulated by neoconservatives as the "demand overload" thesis (Crozier, Huntington, and Watanuki 1975). According to pluralist theory, democracy requires a balance between conflict and consensus (Lipset 1960; see also Alford and Friedland 1985, chap. 3) or, in other versions, between activism and apathy (Berelson, Lazarsfeld, and McPhee, 1954, chap. 14; Sartori 1987, 118–23). A competitive democratic process can produce a working government, on this view, only if there is an underlying consensus about the rules of the game and only if participants are willing to accept compromises and losses. Apathy and nonparticipation turn out to be functional for democracies because they limit the number of conflicting interests that democratic processes must aggregate, thereby lending stability to the system as a whole. Crozier, Huntington, and Watanuki warn that increased activism "disaggregates" interests by challenging elites who manage demand levels by filtering, articulating, and aggregating interests (1975, 165). Such challenges are likely to destabilize the very governments designed to be relatively responsive to popular pressure. Against this possibility, expansive democrats must show that extending democracy transforms interests in ways that allow for, or even increase, governability—at least where there are not deeply rooted structural conflicts of interest.

There are, of course, other objections to expansive democracy;[2] but these four follow most closely from the view of the self as a maximizer of prepolitical interests in a world of scarce resources. It is important to note, however, that it is not enough for expansive democrats to point to the implausibly static view of the self within standard liberal-democratic theory. There are two more reasons why standard liberal democrats might hold this view of the self even if they were to see it as inaccurate or incomplete.

A fifth objection to expansive democracy has less to do with the accuracy of assumptions about the self than with their proper role in democratic theories: a viable theory ought to build on the worst possible limiting cases. Once again, the point is Hobbesian: even if most people are not self-interested maximizers of prepolitical values, institutions based on more optimistic assumptions would fail even if only a few people act in this way. Expansive democrats do not take into account the political viability of inclusive institutions. Against this objection, expansive democrats need to show that they can retain the protective dimensions of standard liberal democracy against the worst possible cases, while also showing that using such cases as a standard for institutional design tends to reinforce just these cases.

A sixth objection to the transformative assumptions of expansive democracy is epistemological. Even if political interactions produced transformative effects, there are few, if any, criteria that could be used to show what an individual's interests would be in the absence of these effects and thus to distinguish desirable from undesirable transformations. To provide such criteria would only be to rely on something more arbitrary than individuals' revealed preferences. To understand political institutions as having an autonomous impact on politics (as expansive dem-

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ocrats do) is to open the door to theories of false consciousness, theories that can justify Leninist vanguardism or Tory paternalism in the name of democracy. To meet this objection, expansive democrats must show that the impact of politics on preference formation cannot be understood without distinguishing interests from revealed preferences, and that such distinctions can be made in intrinsically democratic ways.

THE SELF-TRANSFORMATION THESIS

From the perspective of expansive democratic theory, these objections are to a large extent based on standard liberal-democratic assumptions about the self, assumptions that close the standard view to the full potentials of democracy. Providing an alternative account of the self, however, has so far proved more difficult than critique. Part of the reason is that expansive democratic theories also suffer from underdeveloped assumptions about the self. Nonetheless, certain of the objections I have listed are dealt with by more recent formulations of the self-transformation thesis, four dimensions of which I shall elaborate.

First, the interests and capacities that define the self are determined not only by prepolitical factors but also by the constraints and possibilities of political institutions. The selves that often seem endemic to liberal democracies—selves characterized by selfishness, apathy, alienation, lack of knowledge, and prejudice—are reflections as much of limited means for meaningful political discourse and participation as of more general limits to the capacities of citizens for self-governance. As Bowles and Gintis put it: "Interests are not causally prior to practices. Rather, the interests of a group will in general depend, among other things, upon the membership of the group, its internal discursive and structural organization, and its location in an ecological system of social actions" (1986, 149). Preferences "are as much formed as revealed in the exercise of choice. Individuals choose in order to become, and the nature of the opportunities given for the expression of choice affects the formation of the will" (p. 138; see also Barber 1984; Elkin 1985; Hirst 1986; Lindblom 1990, 17–27; Sunstein 1991).

Second, transformations of the self are important for expansive democrats because they view democracy as justified not so much because it allows maximization of prepolitical wants or preferences as because it maximizes opportunities for self-governance and self-development (Dahl 1989, chap. 7). That standard liberal democrats view "preferences as given allows us to recognize democracy's contribution to the proper aggregation of wants through the counting of votes, but it obscures the contribution of democratic institutions to human development—their unique capacity to foster in people the ability intelligently and creatively to control their lives" (Bowles and Gintis 1986, 123). Similarly, C. B. Macpherson (1977) distinguishes between the image of the self as a consumer in the Benthamite and pluralist traditions of standard liberal democracy and the self as an exerter and developer of capacities in participatory traditions. And Carol Gould (1988) argues that democracy is valuable primarily because humans value both activities that allow them to grow and develop and control over processes of growing and developing. Standard liberal democracy does not, of course, a priori rule out that individuals might be motivated by desires for self-development and self-governance. Indeed, a key justification of standard liberal democracy is that it secures private space within which such values can be realized. What distinguishes expansive democracy is the view that political participation is necessary for self-governance; self-governance is not simply a "private" matter.

This dimension of the self-transformation thesis, however, is particularly vulnerable to one objection I have referred to, namely, that a strong relationship between individual and collective governance is inherently oppressive, especially when conceived in terms of Rousseau's view that individuals become free and self-governing when they replace a plurality of different wills with a generalizable one. Some recent theorists of expansive democracy (especially those influenced by poststructuralism) have sought to recognize this difficulty without giving up the linkage between democracy and self-governance that is essential to the self-transformation thesis. Not unlike pluralists, they argue that conceptions such as the general will indefensibly include an assumption that a world of unified interests exists behind their apparent diversity (Connolly 1988, chap. 3; Connolly 1991; see also Hirst 1986). Translated into a political ideal, the general will produces an insensitivity to difference. To be sure, we may create a general will on any given issue, but it is just as likely that collective decisions will diverge from even the most considered individual preferences. In these cases, individuals maximize self-governance by identifying these differences, not by viewing the general will as their own.

But if expansive democrats give up the Rousseauian understanding of self-governance based on an identity of individual and collective will, does not democracy become simply the equal chance to influence collective outcomes, as in the standard approach? Expansive democrats increasingly argue that there remains an important relationship between democracy and self-governance that does not depend on identifying individual and collective wills, a relationship missing in the standard approach. Democratic participation has a unique capacity to foster and develop *autonomy*, a key element of self-governance (Benhabib 1986; Graham 1986, chaps. 5–6; Habermas 1989; Held 1987, 267–77). Autonomy describes a capacity of judgment: individuals are autonomous if their preferences, goals, and life plans are not the result of manipulation, brainwashing, unthinking obedience, or reflexive acceptance of ascribed roles but, rather, a result of their examining and evaluating

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wants, needs, desires, values, roles, and commitments (Dworkin 1988; Frankfurt 1989; see also Dahl 1985; Elster 1983, 15–23; Mansbridge 1980, 24–25; and Warren n.d.).

Although standard liberal-democratic theory places a high value on autonomy, it conceives autonomy as a prepolitical capacity, maximized by clear separations of private and public life. For expansive democrats, however, autonomy is an inherently social capacity that individuals develop through their interactions with others, by coming to know others both as separate human beings with their own unique capacities, problems, and interests and as beings with whom one shares at least some experiences, problems, and interests. Ideally, everyday interactions would cultivate within individuals the capacity to distinguish the wants, desires, and commitments that lend coherence to their identity from the wants, desires, and commitments that they have, perhaps uncritically, accepted from their culture and may experience as a source of unhappiness. The process of self-discovery (or self-creation) should not, however, be viewed as reinforcing narrow selfishness. On the contrary, a narrowly selfish individual would probably discover a life entangled with others in ways he or she previously did not understand and an identity dependent on commitments and responsibilities to publics, communities, and groups in ways he or she previously did not recognize.

Nor does autonomy imply that one could engage in critical judgment in isolation from others. This is why expansive democrats see participation as important: democratic discourse, negotiation, challenge, compromise, and consensus building foster autonomy. Autonomy is not only desirable for its own sake but essential to the mechanisms of representation and voting that make large-scale democracies possible, since the legitimacy of these mechanisms depends on people's knowing what they want (Sunstein 1991, 10–13).

The expansive argument has become more Tocquevillean than Rousseauian: democracies ought to distribute power in ways that multiply public spaces. These, in turn permit claims and arguments to be publicly tested, altered, and justified (Habermas 1989). This formulation takes a step toward meeting the objection that there is no way to distinguish between preferences and interests without paternalistic or Leninist conceptions of false consciousness. Only democratic implications follow from the view that individuals have interests (no matter what their preferences) in institutionalized processes that enhance their autonomy and self-development (Warren 1990).

A third dimension of the self-transformation thesis is closely related: democracy has an intrinsic, as well as an instrumental, value. Although some values of democracy are means to nonpolitical values, others grow out of democratic processes themselves. Participation completes individuals, in part by enabling them to discover and develop their public dimensions, in part by providing the kinds of interactions that develop capacities for autonomous judgment. Jürgen Habermas has developed this dimension of the self-transformation thesis as carefully as anyone, arguing in *The Structural Transformation of the Public Sphere* (1989) that liberal democracy requires—but is increasingly devoid of—public spheres in which opinions can be put to the test of discourse and changed, and public wills formulated. The point of democracy, in Habermas' view, is not simply to reconcile conflicting interests (as in standard liberal democracy) but to design institutions that encourage discourse, which, in turn, is necessary to identify and distinguish plural, common, and emergent interests (Habermas 1975, 108). Without the experience of argument and challenge within democratic public spheres, individuals will have little sense for what relates them to, and distinguishes them from, others; and this deprives them of an essential condition of self-development. By raising one's wants, needs, and desires to the level of consciousness and by formulating them in speech, one increases one's sense of identity and autonomy—aside from any advantages that might accrue from the substantive outcomes of collective decisions. To the extent that democracy is rooted in discourse, it produces (not just mediates) individual and public wills—an idea with roots in Tocqueville, articulated by J. S. Mill and Dewey, and finding contemporary expression not only in Habermas, but also in Hannah Arendt and others (Barber 1988; Benhabib 1986; e.g., Cunningham 1987, 189–90, 261–63).

The self-transformation thesis includes a fourth argument as to why we should expect increased democracy to enhance governability rather than (as in the standard view) threaten the overload and breakdown of democratic processes. On the expansive view, the "demand overload" thesis overgeneralizes from contemporary liberal democracies, which lack institutional encouragements for transformational effects and rely almost exclusively on adversarial mechanisms that encourage conflict (Barber 1984; Burnheim 1985; Habermas 1989; Mansbridge 1980; Offe 1984, chap. 2). In contrast, increased participation is likely to encourage substantive changes in interests in the direction of commonality, transforming conflict in the direction of consensus. Assuming that structural causes of conflict (inequalities of wealth and power) could also be mitigated, these transformations are most likely to occur within frameworks that encourage interaction and discussion: "The unperceived effects of participation upon individuals' outlook and personality, and upon their development into social beings—make the cost-benefit calculus for self-interest inappropriate" (Bachrach 1975, 52; see also Barber 1984, 151; Graham 1986, 152). But even if individuals fail to discover common interests, they are likely to learn about reciprocity (Gould 1988, 296–97, 303–4); and failing that, they may learn about tolerance. As Frank Cunningham puts it: "The values of respect and tolerance are initially learned by people seeking the same goals but differing on means, and they can then be extended to respect and tolerance for people

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sharing some goals but disagreeing on others. Democracy-inhibiting prejudices (racial, sexist, or national chauvinist) thrive on mutual isolation but begin to be called into question in the interaction that participation in joint projects facilitates" (1987, 76–77; see also Barber 1984, 153). For these reasons, we should expect democratic participation to transform individuals in ways that increase both governability and autonomy.

STRENGTHS AND LIMITATIONS OF THE SELF-TRANSFORMATION THESIS

The successes of the self-transformation thesis are primarily critical: the thesis points up the assumptions about the self in standard liberal democracy that close off explorations of democratic possibilities. As a set of positive claims, however, the thesis is not sufficiently developed to support an alternative democratic theory. For example, it is doubtless true that participation enlightens individuals, since it is hard to engage in an activity and not come to know more about it. But it has not gone unremarked that an active citizenry might become more enlightened without becoming more public-spirited, appreciative of commonality, and tolerant of difference (Dahl 1989, 92–93; Held 1987, 263; Mansbridge 1990b). Under what conditions would democratic interactions produce reciprocity, tolerance, and interest in social goods, rather than, say, providing new ways of politicizing resentment and intolerance? Why should individuals not simply become more effective at pursuing narrow self-interests? Similarly, we know that discourse is a good way to resolve political issues; but why should we also expect it to transform conflicting interests, rather than simply communicating preferences?

Questions such as these are all the more compelling when we consider the pressures of politics against self-transformation that I have noted. We have good reasons to expect consensual decision making, reciprocity, tolerance, and equal respect in institutions such as voluntary organizations, closely knit communities, friendship, and some families. And we tend to see these institutions as pathological when they do not cultivate such values. But politics has an unchosen and conflictive character and often proves to be an environment hostile to transformations such as these. In politics we are thrown together because we live at a particular time in a particular place and because we are confronted with common imperatives for action. Even under the best of circumstances, discourse is likely to be limited in duration and thus in transformative potential—if not because of immediate imperatives, then because there are so many imperatives worthy of discourse and time itself is a scarce commodity (Elster 1983, 37–40; Sirianni 1981, 62–63). No wonder standard liberal democrats presume conflict, then read their concept of the self off of this presumption! No matter how impoverished the concept of the self in standard liberal democracy, it may still seem more appropriate for the *political* sphere than alternative conceptions, not so much because of qualities of the self as a whole as because politics seems to select for precisely its fixed, possessive qualities.

It is unlikely that expansive democracy can deal satisfactorily with these issues as long as it lacks an account of the variety of interests and entanglements that link self-definition to public life. I do not mean that there is no image of a democratically transformable self in these theories. On the contrary, this image is increasingly sophisticated: such a self would be autonomous yet social, individuated yet defined by nonconflicting interests, rational but embodied in numerous different social relations, expressive of individuality yet public in orientation. I doubt neither that such selves are possible nor that we often fail to recognize these dimensions of the self when they in fact develop. The problem is that there is as yet no account of the self that would support this image as it relates to politics. What we find instead are various reactions to the privatized and static conception of the self in standard liberal democracy, each of which tends to emphasize one or more dimensions of the self missing in the standard image. The result is too often an image of the self that mirrors the standard image and is thus unable to reformulate those activities and spheres of interest from which the standard image draws its strength.

The Oversocialized Self

The most widely shared reaction to the privatized concept of the self is to emphasize the inherently social character of the self. Expansive democrats point out that the self is constituted by social relations, identities, and supports. It develops through its social relations and practices, any one of which might provide occasions for mutual interest. Whereas the standard view is that political actions reflect prepolitical interests, expansive democrats point out that different kinds of social practices and identities produce different definitions of interest. Standard liberal democracy reifies outcomes in which individuals are formed by hierarchical or anomic social relations. Were these relations to change, replacing bureaucratic hierarchies and anomic market relations with democratic relations (wherever feasible), self-identities would change in ways that underwrite expansions of democracy (Barber 1984, 153; Bowles and Gintis 1986, 150–51; see also Sunstein 1991).

In the best cases the socialized conception of the self does not rely on an identity between the individual and the collective (as might a traditional conservative or a communitarian view). Nor does it rely on a mechanistic account of social determination such that individual capacities for autonomy become incomprehensible. As I have suggested, these are problems for which there exist defensible positions in theories of expansive democracy. But behind even the best cases is often an assumption that demonstrating the social

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origins, relations, and dependencies of the self is sufficient to show that conflicting interests are transformable into common ones.

The assumption is based on two kinds of conflations. The first conflation is the idea that social interdependencies reflect mutual interests, which in turn provide a basis for self-transformation in the direction of common interests. Of course almost everyone would recognize that there are many social relations within which individuals are mutually defined without having mutual interests (e.g., relations of domination). But there are also interests that despite their being encumbered with social interdependencies, cannot be transformed in nonconflicting directions without oppressive consequences. This is because they are defined (as standard liberal-democratic theory assumes) by scarce goods or goods that by their very nature can only be enjoyed by individuals or small groups. Self-defining interests have a potential for politically significant commonality only where the value of their defining goods depends on large-scale interaction or where they can only be provided in common. This is not to say, of course, that we do not have common interests in agreeing to distributive principles for goods that are intrinsically conflicting but only that in doing so we ought to mediate, rather than transform, interests in these goods. The point suggests that assuming the possibility and desirability of self-transformation in some spheres of interest does indeed risk the oppressive commonality that standard liberal democrats (as well as postmodern pluralists) fear. Expansive democrats should assume that conflict is a constant potential in such spheres and that it is undesirable to mitigate these conflicts except through impersonal mechanisms (see Mansbridge 1980).

The second conflation is a tendency to argue from the reciprocity and solidarity evident in many social relations to politics without recognizing the distinctiveness of political interactions. The view that the self is socially constituted in ways that produce mutual interests is supported mostly by the evidence of child rearing, schooling, friendship, voluntary organizations, workplace cooperation, and various kinds of group solidarity in collective actions. These kinds of social relations do provide important insights into transformations of the self, and they suggest that the range of possible transformations of political interests is far wider than standard liberal democracy assumes. But the analogy between social and political relations is complicated by the fact that these social relations are usually based on common interests (and self-select for common interests), so that conflict is often exceptional and (when it does arise) may be resolved simply by reaffirming common interests. In contrast, we tend to categorize social relations as "political" just when they have a potential for conflict in the face of necessary action. In such cases, common interests have to be created and discovered against a background presumption of conflicting interests. Social and political relations are indeed closely connected; but they are not homologous, suggesting that the self-transformation thesis needs to be reconceived in terms of the distinctive qualities of politics. A beginning would be to classify self-defining interests according to their differing potentials for commonality, conflict, and transformation.

The Overly Discursive Self

A related but distinct innovation in expansive democracy is the notion that the self is discursively constituted. If the self is discursively constituted, then the linguistic medium of politics has transformative potentials that do not show up in standard liberal-democratic theory. Consistent with its origins in the Enlightenment, the standard approach places a heavy burden on language by viewing argument as a substitute for power in collective decision making (Habermas 1989). But it also views language in instrumental terms: at most, language can express prepolitical interests and facilitate discoveries of prepolitical commonalities. Because the standard view does not see language as having constitutive features, it cannot understand how language might create new interests through its many roles in constituting the self.

In contrast, expansive democrats often argue that language serves not only the instrumental functions recognized in standard liberal democracy but also serves to explore mutuality, secure noncognitive relations of affection and affiliation, differentiate and maintain individual autonomy, express self-identity, and reformulate interests (Barber 1984, chap. 3). Each function suggests that language has transformative capacities rooted in its pervasive role in translating internal desires into consciousness and consciousness into something that can be communicated to others. As Habermas puts it:

> Language functions as a kind of transformer; because psychic processes such as sensations, needs and feelings are fitted into structures of linguistic intersubjectivity, inner episodes or experiences are transformed into intentional contents—that is, cognitions into statements, needs and feelings into normative expectations (precepts and values). This transformation produces the distinction, rich in consequences, between the subjectivity of opinion, wanting, pleasure and pain, on the one hand, and the utterances and norms that appear with a *claim to generality* on the other. (Habermas 1975, 10; emphasis original).[3]

Considerations such as these are enormously promising for the self-transformation thesis. They suggest an intrinsic relationship between the linguistic medium of politics and a community of understanding that involves shared commitments—even as conditions for the instrumental and manipulative uses of language typical of politics (Habermas 1984–87, vol. 1). The mistake that theorists of expansive democracy are prone to make in incorporating insights such as those of Habermas, however, is to read a teleology of common interests out of the commonalities inherent in communication. Habermas himself

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does not presume common interests (1975, 108); but when Habermasian views of transformative potentials of language are combined with the view that the self is discursively constituted (e.g., Benhabib 1986, 338), then the conflation follows automatically. Similar consequences follow from relating poststructuralist views of self and language to democratic theory (e.g., Laclau and Mouffe 1985, chap. 4).

The notion that the self is discursively constituted is important but incomplete. It is one thing to note that self-identity is forged in the medium of language but another to identify the self with its linguistic constitution. Identity cannot escape language, and all identity may be (as poststructuralists argue) linguistic in nature. But the self is not just what we can articulate linguistically as its identity. It is also constituted by inarticulate strivings, desires, needs, compulsions, and sentiments, as well as connections to the world that escape language, such as nonverbal communications and sensuous, aesthetic, and spatial relations. One of Freud's most important insights (one that does not depend on the success or failure of psychoanalysis) was that every articulated self-identity leaves a residue. There is always a dislocation between the part of the self made up of an ensemble of desires and strivings and the articulate self, whose identity is formed by the roles that a prevailing discourse with its associated power relations has to offer (citizen, soldier, patriot, mother, boyfriend, caretaker, worker, boss), as well as by everyday rules of social interaction. In Freud's view, residues of the self are in fact signified—but in ways that make them incommunicable. Their significations are scrambled and nonsensical, an effect of their nonidentity with the public domain of language. Yet these dislocations can be accessed through discourse precisely because one's identity will indicate dissatisfactions not only with the fit between one's communicable identity and inarticulate discomforts of the body but within language itself—often through the strivings of the body to become articulate (Gendlin 1987).

We find parallel processes at the political level in discourses that marginalize groups with distinctive experiences, life-styles, and misfortunes (Foucault 1979; Scott 1985). Discursive marginalization may produce adjustments—attempts by these groups to normalize themselves by identifying with a dominant discourse. But such processes will also produce experiential residues that might become resistances. This is where, in fact, we find the importance of democratic discourse in relation to self-transformation. It is not so much that discourse constitutes the self as a subject of interests as that it provides successive identities, which in turn produce new residues and new interests (see Connolly 1991). Discourse can test strivings and desires; play on contradictions and discomforts; and thus articulate, distinguish, and transform interests. But discourse is also bounded by, and partly dependent for its meaning upon, nondiscursive relations to the world and incommunicable inner strivings. Just for this reason we should not expect that the intersubjective qualities of language will, by themselves, constitute selves that are possessed of generalizable interests. Language does not have these powers; and that is why a theory of discourse is not a theory of self-transformation. We can conclude only that the discursive dimensions of politics provide a medium of self-transformation should there be pressures, interests, motives, or strivings in that direction. This locus of discourse is neither appreciated by standard liberal-democratic theory nor properly articulated by expansive democratic theory.

The Underpluralized Self

Theorists of expansive democracy are often guilty of a silence on the question of how pluralistic self-identities relate to common interests and imperatives for political action. More specifically, under what conditions does distinctiveness generate conflict? Theorists of expansive democracy are right to assume that it is not necessary for distinctiveness to generate conflict and to reject the terms in which the issue is couched in standard liberal-democratic theory, that is, the view that individual and common interests have a zero-sum relation to one another except in cases where there are cooperative solutions to maximizing individual preferences (Cunningham 1987, 186–93; Elkin 1985). Moreover, theorists of expansive democracy are right when they make the Hegelian point that even distinctions are products of commonality. One must learn a common language to say distinctive things; one must enter into reciprocal relations of support with others as a condition of developing one's own capacities.

But expansive democrats too often assume that because commonality is necessary to difference, conflicting interests can be transformed into common interests without sacrificing pluralism (see Hirst 1986, chap. 1). This assumption is found, for example, in Bowles and Gintis's argument that markets ought to be eliminated because they allow problems to be solved through "exit" from collective decisions, rather than "voice"—thus undermining a "vibrant democratic culture" (1986, 135). There are many things wrong with markets; but that they allow for alternative ways of solving problems is not one of them. Bowles and Gintis's argument is antipluralistic because it assumes that underlying commonalities are so much more important than differences that they justify political blocks to exit from politics (see Cunningham 1987, 46; Elster 1983, 37).

Assumptions such as these keep us from asking key questions about the relation between commonality and pluralism. When does commonality repress desirable and politically benign differences? When do differences produce antagonisms that demand political responses? Can political life generate and sustain desirable and nonantagonistic distinctions? Sorting out differences, locating their associated values, and identifying the conditions under which they are antagonistic is essential if theories of expansive democracy are to do more than indicate the value of plural-

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ism (see Connolly 1991). Doing so requires an account of the self that relates interests to commonality and plurality.

THE SELF AS A LOCUS OF INTERESTS

I shall offer some preliminary considerations about how we might think about issues of the self in democratic theory. I do not offer a theory of the self but (less ambitiously) a problematization of the self from a political perspective, oriented toward the issues of conflict, commonality, and consensus that divide the standard and the expansive approaches to democracy. I shall remain within the liberal-democratic tradition of conceiving the self as a locus of interests. The concept of *interest* will remain useful for democratic theory, however, only if we take into account certain of its attributes that are often lost in contemporary usage.

First, the term is irreducibly normative. Identifying the term with revealed preferences (as do many standard liberal-democratic theorists) removes the normative content of the concept in ways that have caused some expansive democrats to argue that it no longer has a useful place in democratic theory (Bowles and Gintis 1986, 149–50). But in spite of recent usage, the term identifies a normative problematic that remains essential. By viewing the self as in some way constituted by "interests," we implicitly view the self in light of its political competence, actual or potential. We say, in effect, that the self ought to be able to identify its needs and wants and communicate them to others (Connolly 1983, 64–68). Conceiving of the self as a locus of interests, then, is closely related to the assumption within democratic theory that individuals have a basic interest in their own autonomy. Recognizing that needs and preferences often do diverge (and that this diminishes autonomy), a theory of democracy should be oriented toward creating institutional environments that encourage the self-examination of preferences that brings them closer to needs (Bachrach 1975, 40; Offe 1985, chap. 7). One might even say that the concept of interests, when elaborated, includes the democratic problematic of self-transformation in the direction of enlightened political competence and autonomy (Dahl 1989, 180; Connolly 1983, 62–70).

Second, conceiving of the self as a locus of interests also locates the self within a structural context of conflict and consensus. An interest is always nonspecific until it is matched to a good, as in "I have an interest in X." If these goods relate to, or depend upon, other humans (and most do), then the interest has a potentially political nature, allowing us to formulate questions about commonality and conflict by looking at differing characteristics of these goods. This formulation also shows that conceiving the self as a locus of interest does not mean that interests express qualities of a prepolitical self. Rather, interests refer to the complex of relations through which the self defines itself by holding in view the goods that constitute these relations.

Although conceiving the self as a locus of interests does allow us to view the self from the perspective of conflict and consensus, it is important to note that this particular language of the self (like all other languages of the self) is limited by its problematic. For example, as my analysis will suggest, looking at the self as a locus of interests may highlight as a problem individuals who seek to satisfy nonconflicting social interests by pursuing conflicting goods (e.g., someone who seeks social recognition and acceptance by buying exclusive cars). But it cannot provide an account either of how such a personality comes into being or of how it might be transformed. It can only identify politically significant spheres of transformational possibilities. For these other tasks, complementary accounts of the self are necessary—accounts base on other kinds of problematics, such as those of welfare, therapy, communication, and morality. These problematics, in turn, require theories of needs, personality, discourse, and moral identity. Thus, in the differentiation of interests I shall offer, I make four general assumptions about the self, each of which would require a theory complementary to the account of the self as a locus of interests.

First, any particular articulation of *interest* is likely to coincide imperfectly with, or even conflict with, what an individual *needs* for a satisfactory life. This is only to say that since the self is not wholly fluid in its constitution, preferences can diverge from needs. Individuals have interests in the states of affairs that are amenable to their needs, and we usually want to insist on this no matter what their preferences are. Theories of needs are, however, notoriously difficult to develop beyond lists of basic requirements for livelihood and self-development (Braybrooke 1987). Fortunately, the self-transformation thesis requires only a very weak theory of needs, one that focuses on the general functional requirements of the self in relation to classes of goods that would satisfy them. This is because democratic theory is not a theory of welfare (although there are associated welfare requirements) but a theory of choice, public decision making, and self-governance. For this reason, the theory ought not to specify needs per se but, rather, (1) locate intrinsic possibilities for conflict and consensus according to interests that potentially express classes of needs and (2) locate conditions under which needs can be articulated and rearticulated (another way of talking about transformations of interests). A stronger account of needs is unnecessary and undesirable, since the point of expansive theories of democracy is to recommend institutional arrangements under which individuals develop control over their own need articulation (what I have referred to as *autonomy*).

Second, I assume that individuals require those mixes of satisfactions that maintain self-identity—a topic that requires psychodynamic, therapeutic, and cognitive accounts of the self; that is, interests are ordered as patterns of attachments that produce a

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functioning personality (Warren 1990, n.d.). Accordingly, I assume that most possibilities for self-transformation emerge from those self-identities that attach nonconflicting needs (e.g., for membership or recognition) to conflicting goods ("membership" in the "family" of prestige car owners).

Third, as I have suggested, self-identity is in part a function of linguistic and symbolic identity since definitions of interest are both enabled and constrained by their media of publicity. A full account of identity-based interests requires a linguistic theory of meaning as it relates to self-constitution (see Habermas 1979, chap. 1; Harré 1987).

Finally, I assume that a description of the self in terms of interests can be redescribed in moral terms, in this way linking democratic and moral theory. This is because self-identity includes ideal representations of the self to the self, anchoring a person's conception of their interests in a moral representation of who they would like to be (Taylor 1989).

An account of the self in terms of interests, then, remains incomplete without complementary theories of needs, personality, language, and moral identity; yet the implications of these theories become clear for democratic theory only when appropriated within a theory of interests.

DIFFERENTIATING GOODS AND INTERESTS

With respect to the self-transformation thesis, the key point of a theory of interests is to distinguish between the self-defining interests whose transformation democratic theory ought to view as possible and desirable and those which cannot be transformed without undesirable results. I propose to do this by means of four dichotomies, which, while very general, will allow us to identify eight classes of goods, each of which present different kinds of transformational problems for their associated interests.

First, we can distinguish between goods that can be enjoyed by *individuals* or small groups of individuals, such as families and friends, and goods that are inherently *social* (e.g., a shared identity, whose value depends on larger-scale social interaction and recognition). What distinguishes an individual good is that it loses value the more widely it is shared even if possessing the good depends on complex social interdependencies. This is obviously the case with material goods (food, clothing, shelter). But it is also the case with nonmaterial goods that emerge from pairings of very small groups (e.g., love and friendship). While love between two people will draw on widely shared social goods for, say, rituals of interaction, it cannot be expanded to love of a city or nation or humankind without becoming a different kind of sentiment. Social goods, on the other hand, depend on broad sharing for their value. Citizenship or identification with a people or community has value only because it is widely shared. Likewise with language: in general, the more broadly shared its structures and conventions, the more value it has to individuals as a means of communication and expression. Of course, few, if any, activities are exclusively individual. For example, while the value of food in satisfying hunger is diminished as the same quantity of food is divided, the activity of eating almost always includes social goods that come into existence only through broad sharing, as is clear on such occasions as Thanksgiving and Seder.

The point of this distinction is that social goods can form bases for transformations away from conflict and toward commonality in ways that individual goods cannot. That a good is individual does not necessarily make it hostile to commonality; but neither should democratic theory seek transformational effects in the spheres defined by these interests. Attempts to transform individual goods in common directions will violate their inherent qualities, produce oppressive effects, and diminish individual autonomy and development. If, however, individuals seek individual goods for reasons that are primarily social (an important case), this will be a promising arena for transformational effects.

Second, we can distinguish goods according to whether they are characterized by *excludability* or *jointness of supply* (*nonexcludability*). Nonexcludable goods can be supplied to single individuals only if they are supplied to everyone (roads, parks, national security, aesthetically pleasing public spaces, clean air). These goods usually require collective action for their provision. The significance of this distinction is that although nonexcludable goods may serve individual interests, in most cases they require cooperation if they are to be provided. This suggests that democratic theory ought to look to arenas defined by nonexcludable goods for transformations of the self, since in pursuing these goods individuals may discover or create common interests. Excludable goods, although less promising, need not lead to conflict unless combined with scarcity. Love, friendship, and some kinds of group memberships are excludable but not inherently scarce, suggesting that they are neither promising arenas of self-transformation in the direction of commonality nor hostile to it.

Third, goods can be either *material* or *symbolic*. Material goods such as food, clothing, and shelter have physical presence and satisfy physical needs, although they rarely satisfy physical needs alone. Symbolic goods (including psychological goods) include recognition, self-identity, and symbolic resources (language, culture, life-style). Except where they combine with excludability, symbolic goods will more readily provide for the emergence of common interests because their value depends upon broad sharing.

Finally, goods can be *scarce* or *nonscarce*. Scarce goods are those for which demand is greater than supply in ways that engender potential conflicts between individuals regardless whether the source of the scarcity is physical (limited supplies of land) or social (a royal title) (see Hirsch 1976, chap. 2). Non-

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TABLE 1

Generating a Typology of Goods That Present Different Kinds of Transformational Problems for Their Associated Interests

	INDIVIDUAL	SOCIAL
GOODS THAT ARE EXCLUDABLE AND MATERIAL:		
Scarce	Individual material goods	—
Nonscarce	Eclectic and plentiful material goods	—
GOODS THAT ARE NONEXCLUDABLE AND MATERIAL:		
Scarce	Public material goods	—
Nonscarce	Natural goods	—
GOODS THAT ARE EXCLUDABLE AND SYMBOLIC:		
Scarce	—	Positional and status goods
Nonscarce	Interpersonal identity goods	Exclusive group identity goods
GOODS THAT ARE NONEXCLUDABLE AND SYMBOLIC:		
Scarce	—	—
Nonscarce	—	Social identity goods

scarce goods are those for which there is a relatively unlimited supply (friendship, self-improvement, language).

There is, of course, a sense in which all goods are scarce because they are costly of time. Thus, pursuing friendships or becoming competent at the use of language often means that other kinds of goods cannot be pursued. This ontological scarcity has important ramifications for assessing trade-offs between goods of participatory democracy and other goods (Sirianni 1981). The distinction I am making here, however, has to do with the potential for conflict in the pursuit of goods. Pursuing friendship or literacy, for example, may be costly of time; but since their supply is limited neither by physical scarcity nor by social scarcity, we would not necessarily expect an increase of conflict were more people to pursue these goods. The significance of the scarce/nonscarce distinction is that shifts in self-defining interests from scarce to nonscarce goods will also reduce potentials for conflict.

Combining these distinctions produces 16 possible classes of goods (see Table 1). Of these classes 8 are empty of goods that are significant for democratic theory, generating a typology of 8 goods and their associated interests, each with different characteristics in respect to self-transformation.

1. Goods that are individual, material, scarce, and excludable I shall refer to as *individual material goods*. These include food, clothing, shelter, and other material consumables enjoyable by individuals or small groups such as families.
2. Goods that are individual, material, scarce, and nonexcludable I shall refer to as *public material goods*. These include public goods such as parks, libraries, roads, clean air, and territorial security.
3. Goods that are individual, material, nonscarce, and excludable I shall refer to as *eclectic material goods and plentiful material goods*. Eclectic goods are those that so few individuals enjoy relative to supply that there is no scarcity (dandelions). Plentiful goods are those in such great supply that there is no scarcity (fruit on a tropical island of natural abundance).
4. Goods that are individual, material, nonscarce, and nonexcludable I shall refer to as *natural goods*. These include attributes of our natural environment (oxygen, sunlight) that are normally in abundance.
5. Goods that are individual, symbolic, nonscarce, and excludable I shall refer to as *interpersonal identity goods*. These include the goods of personal identity that emerge out of close interpersonal relations such as love and friendship. (In principle these goods are not scarce, since every new relationship brings new goods into being without this necessarily leading to conflict; but they are excludable, since individuals partake of different interpersonal goods whose origins are intimate and not societywide.)
6. Goods that are social, symbolic, scarce, and excludable I shall refer to as *positional and status goods*. Positional goods are material goods (houses, cars, clothing) whose value is primarily symbolic and based on a broadly recognized identification with class or status. Status goods are symbolic goods

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TABLE 2

A Typology of Goods That Present Different Kinds of Transformational Problems for Their Associated Interests

	QUESTIONS RELATING TO THE TRANSFORMABILITY OF INTERESTS		
GOODS	IS THE GOOD NONSCARCE AND THUS INHERENTLY NONCONFLICTING?	DOES THE VALUE OF THE GOOD DEPEND ON SOCIAL INTERACTION AND RECOGNITION?	DOES ACHIEVING THE GOOD REQUIRE COMMON DELIBERATION AND ACTION?
Individual material	No	No	No
Eclectic and plentiful	Yes	No	No
Natural	Yes	No	No
Interpersonal identity	Yes	No	No
Positional and status	No	Yes	No
Exclusive group identity	Yes	Yes	No
Public material	No	No	Yes
Social identity	Yes	Yes	Yes

(degrees, titles) that have value because they are broadly recognized but also excludable and scarce (see Hirsch 1976).

7. Goods that are social, symbolic, nonscarce, and excludable I shall refer to as *exclusive group identity goods.* These include goods of self-identity that depend on group distinctions (based on religion, language, and ethnicity or on special interests, skills, professional and workplace associations, fads, and life-styles) but do not depend on scarcity, since there is no inherent shortage of group identities (as there must be with, say, status identities).
8. Goods that are social, symbolic, nonscarce and nonexcludable I shall call *social identity goods.* These include communal resources (language, national culture), as well as identities and recognitions that attach to the goals, ideals, activities, and dialogue that constitute a society.

We can now arrange these goods and associated interests according to questions relevant to self-transformation. In Table 2, I ask three questions of each good: (1) Is the good nonscarce and thus inherently nonconflicting? (2) Does the value of the good depend on broad social interaction and recognition? (3) Does achieving the good require common deliberation and action? The goods are loosely ordered according to their compatibility with self-transformation. The three *nos* of individual material goods indicates that this is not a promising arena for self-transformation. The three *yeses* of social identity goods indicates that a democracy might encourage transformations of the self based on these goods without threatening autonomy. The mixed cases require qualification.

Note that the standard model tends to assimilate the characteristics of all goods to individual material goods, in this way failing to recognize the interests upon which expansions of democracy might build. On the other hand, expansive democrats tend to assimilate the characteristics of all goods to those of social goods, overlooking arenas in which demands for self-transformations are undesirable or misplaced.

Beginning at the top of the table, individual material goods have few inherent properties of commonality. They need not be supplied collectively, and they are potentially scarce. The demand for self-transformation in the direction of commonality here can only produce a collective denial of these interests. Individuals will find political demands for self-transformation in this arena to be oppressive, and they will experience commonality as self-sacrifice. What is called for, instead, is a common recognition of allocative problems, a recognition resulting in distributional mechanisms that operate according to generally agreed rules and principles.

These goods are, of course, the home domain of standard liberal democracy. Yet even here, the standard approach is inadequate. First, because it generalizes the allocative model of politics based on an assimilation of all interests to individual material interests, the standard approach is unable to appreciate situations in which a community might choose to serve certain common, social interests (e.g., when individuals view the poverty of others as inconsistent with their view of a good society) by agreeing on an appropriate method of allocation (e.g., modifying the workings of markets through the tax structure). These sentiments can develop, however, only because selves are partly defined by common, social interests (see Sunstein 1991).

Second, the standard approach is unable to identify situations in which individuals attempt to satisfy nonconflicting interests through conflictual goods, situations for which the self-transformation thesis is entirely appropriate. When two working parents compensate for the attention they deny their children by bringing home gifts, they introduce a relative scarcity of interpersonal goods and displace these onto individual material goods. Their children may later seek love and recognition through individual material goods, a confusion that reduces their auton-

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omy and satisfaction. The confusion also increases potentials for conflict, not only because of the scarce nature of the goods they seek but also because their lack of satisfaction is likely to produce resentments that fuel the politics of scapegoating. We could even define a consumer culture as a set of social structures, institutions, and interpretations that displaces desires for nonconflictual, nonscarce goods onto individual, scarce, material goods (see Leiss 1976). Thus, even though individual material interests are not appropriate for self-transformation, increasing democratic dialogue and social interaction would provide opportunities to distinguish between interests that are irreducibly individual and material and interests that are inappropriately displaced into this arena. These are the cases in which expanded democracy might transform individual material interests in common directions while developing selves that are increasingly autonomous and have increasingly diverse possibilities for satisfaction.

Eclectic and plentiful goods and natural goods are similar in that they neither are inherently conflictual nor depend on commonalities for their existence or satisfaction. All other things being equal, they ought to fall outside of institutionalized politics, democratic politics included—which is to say that they ought to be politically protected but not politically allocated or transformed. Eclectic and plentiful goods—excludable goods of interest to only a few individuals or in such great supply that there is no competition for them—present no problems for politics and ought to be left alone, owing to their contributions to pluralism. Natural goods such as sunlight and breathable air present political problems only in the sense that they are necessary for all individuals yet can be spoiled by a few individuals because they are nonexcludable. When damaged, they in effect become scarce and present the problems of public goods.

In standard liberal democracy interpersonal identity goods are recognized under the guise of private protections (Warren 1989). Because the standard view assimilates these goods to possessive assumptions, it also assumes they are inherently conflictual. But there is no reason for them to be conflictual in principle because they are nonscarce: every social relation generates new identity goods—goods that did not exist before and that are emergent properties of the relationship. The exclusiveness of love and friendship often leads to conflict, as will a lack of reciprocity in recognitions. These conflicts are not, however, "political" in the sense that they cannot be addressed directly by collective actions: they do not present problems of allocation; nor can interpersonal recognitions be created politically, that is, outside of intimate relations.

This is not to say that these goods are not affected by contingencies of a political nature or that they might not be subsumed inappropriately into politics. It is here that questions about self-transformation become important. To return to a previous example, when economic circumstances force parents to work full-time, they may not be able to devote the time to their children or to each other that their relationships require. In such cases, interpersonal recognitions become scarce, and a collective failure to deal with distributive justice is reflected in a sphere of intimacy. Unsatisfied interpersonal interests may then be displaced into arenas with intrinsically conflictual properties (e.g., material, positional, or status goods) or even into politics itself. In the latter case, membership in a nation, ethnic group, or movement may vicariously substitute for scarce interpersonal relations. Such displacements are likely to lead to frustrations, since the good is inappropriate to the interest. If so, this would indicate reduced autonomy for individuals, as well as contribute to the conflictual politics of scapegoating, racism, and resentment.

In the arena of interpersonal identity goods, then, self-transformations can and ought to occur not by reorienting interpersonal identities toward democracy or seeking therapeutic effects from politics but by reinforcing the conditions (e.g., time and protected intimate space) that allow interpersonal identities to develop. In these cases, transformations of the self occur not through democratic interaction but within protected and enabled spheres of intimacy. Although these transformations are not achieved by directly political means, they are nonetheless of key importance to expanded democracy since we might expect them to reduce conflict by reducing the displacement of frustrated interpersonal interests onto conflictual goods.

Positional and status goods are the only social, symbolic goods that standard liberal democracy recognizes—perhaps because they are inherently conflictual, consistent with the assumptions of the standard view. Positional and status goods are both social and scarce because what makes them worth having is their widely acknowledged scarcity. Mercedes Benzes and medical degrees both have intrinsic merits; but when people pursue them because they are widely valued and relatively uncommon, they become positional and status goods and take on additional conflictual dimensions. Unlike individual material goods, positional and status goods are appropriate arenas of self-transformation because they are often avenues for pursuing goods that are not inherently conflictual. In a society (such as ours) where most social organizations are neither democratic nor egalitarian, a broad range of nonconflictual interests in differentiation, recognition, and personal identity has a narrow, often conflictual range of institutional means of expression and development. In these arenas of interest, expanding democracy might very well produce desirable transformations of the self.

In the arenas of interest constituted by exclusive group identity goods are found the most bitter, uncompromising, and explosive conflicts. Whereas individuals will usually negotiate and compromise individual material goods, they are often willing to die for their group identity. The irony is that exclusive group identity goods are not inherently conflictual because they are not scarce, in spite of the fact that not everyone in a society will identify with the

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same groups. As with interpersonal identity goods, every new group identity produces new goods without necessarily diminishing the goods available to others (as with individual material goods) or depending upon invidious comparisons (as with positional and status goods). These group interests are appropriate for self-transformation—not to produce common identities out of plural ones but to transform different identities in ways that produce diversity and pluralism rather than stratification and conflict.

As in the case of positional and status goods, expansive democrats should explore the possibility that conflicting group identities are produced by social structures and personal biographies that link group identities to inherently conflicting goods. In the case of racism, for example, differences in culture, life-style, and physical appearance determine differences in access to jobs, housing, and education, linking nonconflictual interests to conflictual ones (in more traditional terms, *race* to *class*). This example suggests, however, that expanding democracy will positively transform the self only where a society also chooses to address problems of distributive justice at the same time: toleration and appreciation of differences are likely to develop only when there are no systematic, differential sacrifices of individual material interests between groups. Otherwise, dialogue will heighten conflict by exposing injustices—a process necessary to future consensus but one that is likely to increase conflict as long as injustices remain in arenas of conflictual goods. The transformational impact of expanded democracy in such cases will depend on how conflictual arenas of interest are arranged.

Other kinds of group conflicts may be less transformable through politics. Sometimes groups are formed through their identification with irreconcilable principles. The abortion issue, for example, may be so tied in with differences between secular and religious worldviews that any self-transformation would have to reach deeply into the organization of the self. If these kinds of differences can be mitigated by dialogue (and this is by no means clear), it would be of a kind unlikely to take place in a political context (Warren, n.d.). But even if some kinds of group conflicts are not likely to be transformed by expanding democracy, it is important to note the costs of adversarial institutions with limited democracy: they encourage political entrepreneurs to create regional, racial, ethnic, religious, and nationalist constituencies, producing conflicts where there may have been only differences (Burnheim 1985; Mansbridge 1980).

Public material goods present unique opportunities for self-transformation when compared with other goods. Although they are inherently conflictual and do not depend on commonality for their value, they can only be gained through common action. Combined with expanded democracy, this characteristic presents individuals with opportunities to change the way they make trade-offs between individual and public material goods, as well as to discover other, nonconflictual goods associated with the common deliberation and action that public goods require.

I have already suggested that social identity goods lend support to the self-transformation thesis. Here I shall add comment on one problematic feature of social goods. They can often be ideologically manipulated by individuals, groups, classes, or organizations who seek to portray their partial, conflicting interests as common ones. Ideological appeals of this sort can be successful just because social goods have a powerful impact upon individual identity (Warren 1989, 1990). Indeed, individuals often find that they must ignore some kinds of conflicts in order to gain social goods like membership, pride of belonging, and recognition. In terms of the distinctions I have developed here, one way of identifying situations in which social goods are used for these purposes would be to look at how appeals to social goods relate to other arenas of interest. In terms of the goods listed in Table 2, the more the goods toward the top of the table are the subject of appeals to commonality, the more one ought to suspect an ideological equation between partial and common interests. If an arrangement and its justification require differential sacrifices for the sake of a social good (e.g., military risk by the economically disadvantaged for "the pride of the nation"), then one should suspect that a social interest is being manipulated ideologically to produce a sentiment of commonality when most other interests are conflicting. This is where communitarians are often culpable, and this is why expansive democrats insist that self-transformation in the direction of commonality must occur through democratic dialogue—the context in which ideologically manipulated commonality is least likely to survive.

CONCLUSION

I have offered a sketch of how theorists of expansive democracy might take on the burden of argument with respect to the self-transformation thesis. The sketch is not yet a democratic theory: it lacks accounts of power, justice, allocation, judgment, and other features of institutional design. But it does suggest new ways of thinking about self-transformation, thus anticipating some of the more important objections to expanding democracy by standard liberal democrats. Qualifying the self-transformation thesis by differentiating goods and interests suggests that some interest arenas have potentials for conflict that must be dealt with through institutions designed for conflict even when there are common interests in resolving conflict. The qualifications also suggest that most differences are inherently neither conflictual nor hostile to commonality. Commonality can coexist with pluralism, however, only where institutions are appropriate to the kinds of interest they mediate and express. Finally, autonomy and self-development are increased by institutions that take into account the characteristics of each kind of interest as it relates to the self. Only by differentiating the social relations

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implied by these interests can we grasp the full potentials of democratic processes and commitments, potentials that are ultimately rooted in how democracy protects and facilitates persons.

Notes

1. My conclusion borrows from Jane Mansbridge's *Adversary Democracy* (1980) in calling for different institutional mechanisms for conflicting and consensual interests but relies on a different argument. Mansbridge argues that standard liberal democracy overlooks consensual interests, but she treats both consensual and conflicting interests as given parameters of institutional processes. In contrast, I focus on processes and constraints that constitute differing configurations of interests as conflicting or consensual. For a critique of Mansbridge on this point, see Pitkin and Shumer 1982. My approach is also loosely indebted to Michael Walzer's *Spheres of Justice* (1983). Walzer distinguishes spheres of value in terms of the goods intrinsic to the practices that define these spheres. This allows him to define domination in terms of the goods of one sphere converting into the goods of another (e.g., the buying of loyalty). Walzer assumes without developing, however, an account of the self that would show how the practices and goods of each sphere enable or disenable politically relevant capacities, such as autonomy.

2. These other objections have to do in part with problematic relations between democracy, expertise, and bureaucracy. See, e.g., Sartori 1987, 113–20. For suggestions, see Gould 1988, chap. 10 and Pollitt 1986. They also have to do with relations between democracy and markets; see Miller 1989.

3. See also Habermas 1984–87.

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POWERFUL PACIFISTS: DEMOCRATIC STATES AND WAR

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Democracies are less likely to fight wars with each other. They are also more likely to prevail in wars with autocratic states. I offer an explanation of this syndrome of powerful pacifism drawn from the microeconomic theory of the state. State rent seeking creates an imperialist bias in a country's foreign policy. This bias is smallest in democracies, where the costs to society of controlling the state are relatively low, and greatest in autocracies, where the costs are higher. As a result of this bias, autocracies will be more expansionist and, in turn, war-prone. In their relations with each other, where the absence of this imperialist bias is manifest, the relative pacifism of democracies appears. In addition, democracies, constrained by their societies from earning rents, will devote greater absolute resources to security, enjoy greater societal support for their policies, and tend to form overwhelming countercoalitions against expansionist autocracies. It follows that democracies will be more likely to win wars.

No less likely to fight wars in general, democracies are significantly less likely to fight each other. The relative pacifism of democracies remains a puzzle. Although it has attracted significant attention, most of the recent work has been empirical (Maoz and Abdolali 1989; Small and Singer 1976) or philosophical (Doyle 1983a). No *theory* presently exists that can account for this striking empirical regularity.

Equally important, but far less widely recognized, is the propensity of democracies to win the wars that they do fight. In view of the drawbacks commonly associated with the democratic conduct of foreign policy,[1] this finding poses even more of a conundrum. If democratic decisionmaking is often slow, inept, naive, and prone to stalemate, how and why do democracies typically triumph over their faster, more professional, sophisticated, and decisive autocratic brethren? In the ultimate contest of national strength and will, why are democracies more powerful?

I offer one possible explanation for this syndrome of powerful pacifism drawn from a larger theory of grand strategy (see Lake 1991). I argue that autocratic states, which typically earn rents at the expense of their societies, will possess an imperialist bias and tend to be more expansionist and, in turn, more war-prone. To the extent that democratic states tend to be more constrained by their societies from earning rents, wage their own wars of expansion under more restricted conditions, possess greater incentives to intervene in the domestic political affairs of autocracies, and become objects of autocratic expansion, there should be no significant overall difference in frequency of war involvement. In the absence of this imperialist bias, however, democracies should be relatively pacific in their relations with each other.

Moreover, democratic states, because they are constrained from earning rents, will tend to create fewer economic distortions, possess greater national wealth, and devote greater absolute resources to national security. They will also tend to enjoy greater societal support for their policies. To the extent that states balance threats rather than power, democracies will also tend to form overwhelming countercoalitions against expansionist autocracies. Together, these three propositions imply that democratic states should be more likely to win wars.

A THEORY OF THE RENT-SEEKING STATE AND FOREIGN POLICY

Stimulated by the pioneering work of Lane (1979), the microeconomic theory of the state conceives of the state as a profit-maximizing firm that trades services for revenues.[2] I define *profit* here as the difference between revenues acquired by the state and its real costs of producing services and collecting revenue. Real costs are determined, in turn, by the fair market value of the resources consumed by the state in the production and collection processes.[3] Thus, following normal usage, profit includes both what is commonly called *normal or economic profit*, defined by the opportunity cost of the factors of production or their return in their next-best occupation, and *supernormal profits or rents*, returns greater than what is necessary to sustain the factors of production in their present use. (See Tullock 1980.)

Foremost among the services provided by the state is protection. For purposes of the following analysis, protection is further restricted to mean only defense from external threats. While I focus on this single "industry," the argument can easily be generalized to other state services; and rents earned by the state on any service in any area under its control are sufficient to create an imperialist bias in its grand strategy.

Many competing definitions of the state exist. In this theory, the state, a service-producing firm, is defined by two functional attributes. Since the protection industry typically enjoys economies of scale over an extensive geographic area, the state forms a natural but local monopoly (Lane 1979, 23). As a result, one and only one state will exist in any area at

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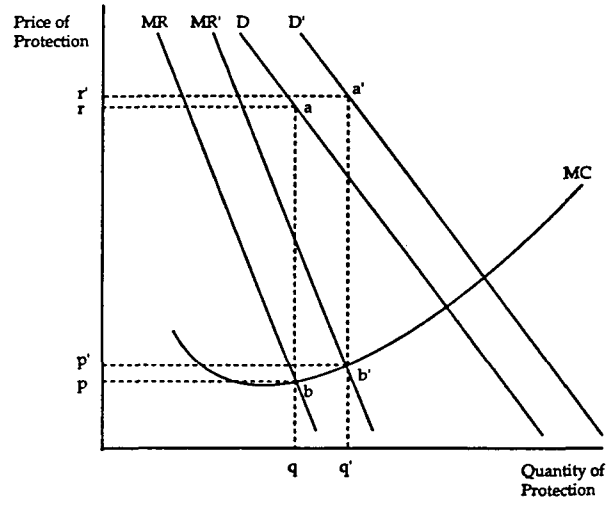
Figure 1

The Supply and Demand for Protection under Monopoly Provision



any given time. Similarly, as protection from foreign threats forms a local public good, whose suboptimal provision is otherwise ensured by the large number of citizens involved, the state will supply this service only if it is granted, or is able to obtain, some coercive ability over its society. These two aspects of the protection service, local monopoly and coercive supply, are consonant with Weber's classic definition of the state as a "compulsory organization with a territorial basis" that monopolizes the legitimate use of force (1978, 1, 58).

The Demand and Supply of Protection

All individuals possess a positive demand for protection; and as price declines, they increase the quantity they demand. Accordingly, the demand curve for protection slopes downward and to the right, as in Figure 1.[4] The precise slope is a function of societal preferences (which are considered to be exogenous) and the availability of substitutes (which, given the nature of protection, tend to be few), suggesting that demand is relatively inelastic and the curve correspondingly steep. Although theoretically possible, it is unlikely that the demand for protection is perfectly inelastic (i.e., that the demand curve is perpendicular to the horizontal axis), since this implies that society is willing to pay virtually any price for even small amounts of the service. To the extent that society places any value on goods and services other than protection, or, if there is any trade-off between guns and butter, the demand curve must possess at least a slight negative slope. It is equally unlikely that the demand for protection can be saturated. Historical experience suggests that even high levels of defense spending do not create feelings of total security. To the extent that the security dilemma holds at all (Jervis 1978), the demand for protection can be sated not at some high level of military spending but, rather, only in a world where everyone else is completely disarmed.[5] In short, under most feasible conditions, the demand curve for protection is sensitive to price.

The level of protection demanded by society, in turn, is primarily a function of the level of external threat. The greater the external threat, ceteris paribus, the higher the demand for protection will be. This is depicted in Figure 1 as a shift outward in the demand curve ($D' > D$).

As in any monopoly, the state enjoys some measure of market power and can, within the limits set by the demand schedule, control the quantity of the good (protection) supplied; and as with any profit-maximizing firm, the state will set its output at the level that equates marginal cost with marginal revenue and charge what the market will bear.[6] In Figure 1, price p represents the normal profit level; but since in all monopolies the profit-maximizing level of production lies below the demand curve, the final price charged to consumers will be higher and may be as high as r. The difference between p and the price charged, say r, defines the rent or supernormal profit earned by the state—represented graphically by the rectangle p-r-a-b.[7]

Unlike other monopolists, however, states can also act opportunistically against their own societies by artificially increasing the demand for their services through extortion or racketeering. Extortion occurs when states magnify, exaggerate, or "oversell" foreign threats to society, whether by supplying incomplete information or engaging in outright deception (see Ames and Rapp 1977; Lowi 1967). States conduct protection rackets by actively creating foreign threats, from which they then protect society (see Tilly 1985). In both cases, a state effectively shifts the demand curve outward ($D' > D$) and thereby earns greater rents (p'-r'-a'-b' > p-r-a-b).

Two important points follow from this analysis. First, the level of protection supplied by the monopoly state will always be less than that produced under conditions more closely approximating "pure competition."[8] While society would benefit from higher levels of protection, given prevailing costs of production, the profit-maximizing or optimal strategy for the state is to restrict supply—whether it can successfully capture the potential rents or not. Insecurity is an inherent feature of life under the modern state.

Second, the state faces strong incentives to seek rents at the expense of society. In other words, the state can benefit itself by charging consumers the monopoly price for protection (r in Figure 1) and by artificially inflating demand through extortion or racketeering.

Societal Constraints on State Rent Seeking

Consumers clearly prefer to purchase protection at the lowest sustainable price (p in Figure 1). The state, on the other hand, clearly prefers to sell protection at the highest possible price, which is determined by the slope of the demand curve and represented by r.

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The actual price—and the level of rents extracted from society—is determined by how well individual citizens can control or regulate the rent-seeking behavior of the state. Society's ability to control the state, in turn, is influenced by the costs of three separate activities: monitoring state behavior, voice, and exit (see Hirschman 1970).

In order to control the state, individuals must first monitor its performance and acquire information on the strategies it is pursuing, its real costs of protection production, the level of foreign threat, and the like. Monitoring the state is analogous to principal–agent problems in publicly held corporations, where the stockholders (the principals) seek to ensure that the managers (the agents) work hard and faithfully in their interests.[9] The "problem" arises because no single stockholder typically has any incentive to invest in, or acquire information on, the manager's true performance—information, of course, it is assumed that the managers themselves possess. Collective action problems also stymie any group investment in information. The higher the costs of acquiring information, the less control the stockholders can exert over the firm and the more the managers can shirk or adopt policies that benefit themselves at the expense of their principals. The same is true of the state–society problem: no single citizen has any incentive to invest in information; and, because of the free-rider problem, collective investment in information occurs only at suboptimal levels.[10] The higher the costs of acquiring information regarding state performance, the greater latitude state officials possess to engage in rent-seeking behavior.

Once performance and the level of state rents have been assessed, individuals have two instruments through which to alter or change state behavior: exit and voice. Through exit, individuals move and deplete both the resource base of the state, raising its real costs, and its market for protection, lowering the price it can charge. Discipline is imposed upon the state, in other words, by reducing its profits, thereby punishing it for undesirable behavior.

As Hirschman notes, exit is the quintessential economic strategy: if a consumer ceases to like a product, he or she simply stops purchasing it (1970, 15). Consumers of state-provided services do not have quite the same freedom. Because protection is a public good supplied by a local monopoly, individuals cannot choose to consume varying amounts or qualities; indeed, because it is coercively supplied, individuals cannot choose whether or not to consume any protection at all. Nonetheless, political exit can occur, although it takes different forms and usually entails a higher cost than exit from private goods consumption. Individuals can choose to migrate or "vote with their feet" (or assets), moving from high-rent to low-rent areas.[11] Freedom of emigration is often one of the first rights obtained in the process of democratization (as was recently witnessed in Eastern Europe), suggesting that the right to exit and democracy will tend to be conjoined, with the former serving as the ultimate guarantor against failures of the latter. Similarly, territories can secede or threaten to secede. If large enough, they can try to form an independent state or, failing that, they can try to merge with other secessionist or democratic territories into a larger union. While possible, exit is costly to the individuals or territories that choose it. The higher the cost of exit, the greater the ability of the state to earn rents.

Voice, or political participation, disciplines the state by separating or threatening to separate state officials from their offices: the citizens stay, but the composition of the state changes. Voice can take many forms, from voting, to campaign contributions, to mass unrest, to active rebellion.

At the individual level, the costs of political participation are unevenly distributed across society. This occurs for a host of idiosyncratic reasons. For instance, not all countries have universal suffrage, the opportunity cost of a campaign contribution is significantly less for a multimillionaire than for a welfare family, and the military can overthrow a leader with greater ease than can unarmed civilians.

At the aggregate or national level, the costs of political participation vary by regime type. For instance, in most democracies, where elections are the primary focus of political participation for the majority of citizens, it is relatively costless to vote and exercise voice.[12] At the other extreme, autocratic states typically suppress political dissent; and voting, if it occurs at all, is ineffective in removing officials from power. In these countries, to replace, or effectively threaten to replace, a ruler requires either mass unrest or some form of armed rebellion—activities that carry considerably higher individual costs, including the possibility of death. In these polities, voice is very costly to the average citizen and, as a result, is seldom exercised. It follows that the higher the costs of political participation, on average, the greater the state's ability to earn rents will be.[13] The relatively low cost of political participation in democracies constrains the state's rent-seeking ability, whereas the relatively high cost of political participation in autocracies frees the state to earn rents.

In determining its level of feasible rents, the state will act as a discriminating monopolist, charging citizens in general and each separate citizen in particular a price for protection positively related to the costs of monitoring, exit, and political participation. Indeed, the state will charge up to—but in equilibrium not more than—the price at which individuals would be tempted either to exit or to engage in the lowest-cost form of political participation available to them that would effectively remove current state officials from power.

Expansion and the Rent-seeking State

In practice, there are always positive costs of monitoring state behavior and exercising voice and exit. As a result, all states possess some ability to earn rents, although the ability will be larger in autocracies than in democracies for the reasons just surveyed. To the

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extent that a state can earn rents, state and societal interests will diverge and the state will be biased toward an expansionary foreign policy.[14] This relationship is continuous. The higher the costs to society of controlling the state, the greater will be the rent-seeking ability of the state, the more the interests of state and society will diverge, and the more expansionist the state will become.[15] This imperialist bias arises for three reasons.

First, expansion may increase the state's rent-seeking ability by reducing the benefits of exit. The net benefit of exit to any individual (and thus that individual's incentive to engage in this action) is determined by both the *push* of high rents at home and the *pull* of lower rents abroad. When all states extract equally high rents, there is no incentive to move. If through expansion a state can eliminate or engulf a low-rent competitor, it increases its own ability to earn rents. This suggests that low-rent states will often be objects of expansion for rent-seeking autocracies.

Second, a state may also expand so as to provoke others into threatening its own society. Both extortion and racketeering rest upon persuading citizens that foreign threats are larger and more real than they are or otherwise would be. If successful, the state convinces consumers to increase their demand for protection and, in turn, earns more rents. Even with incomplete and costly information, however, citizens may eventually discern the true level of threat and lower their demand. Through expansion short of universal empire, the state lends credibility to extortion and supports racketeering, thereby strengthening its ability to earn rents at the expense of society.

Third, and most important, the larger the state's rent-seeking ability, the higher the total revenue earned by the state. The more revenue (ceteris paribus), the larger the optimal size of the political unit. These relationships are depicted in Figure 2.

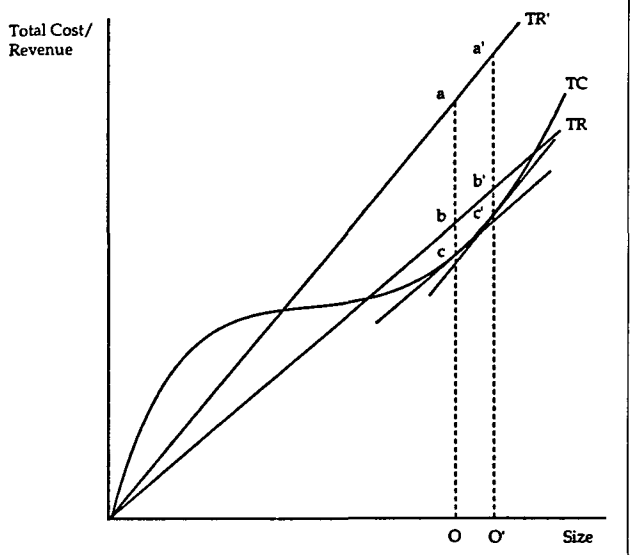
For all states, an optimal size exists defined by the costs of collecting revenue and producing protection and the revenues earned by providing this service to society. Each additional unit of territory acquired by the state produces additional revenue: the state becomes the new local monopoly supplier of protection, and it taxes consumers in that region accordingly.

On the other hand, the costs of governing rise with the size of the political unit, placing an effective cap on the size of nation–states. These resource costs occur primarily in the form of transactions costs of revenue collection (see Levi 1988; North 1981). Over some limited range, the state may enjoy increasing returns to scale in revenue collection; but soon, the addition of more territory begins to strain the administrative abilities of the state, leading to diminishing returns.

When combined with the costs of producing protection, the state's total cost curve typically resembles that in Figure 2. Economies of scale in protection, in conjunction with initial increasing returns in revenue collection, suggest that the slope of the total cost-curve declines for some substantial distance, flattens

Figure 2

The Optimal Size of Political Units



as the marginal costs of revenue collection begin to rise more rapidly, and eventually increases as the costs of revenue collection accelerate and the economies of scale in protection are exhausted.

If revenues increase monotonically, there is a single optimal size of the political unit where marginal revenue equals the marginal costs of collection and production. Geometrically, this occurs where a line tangent to the cost curve is equal to the slope of the revenue line, as at size *O*. At *O*, the economic profit to the state is measured by the line segment bc. No profit-maximizing state has any incentive to expand beyond the point where marginal cost is equated with marginal revenue.

Rents earned by the state, however, cause the total revenue line to rotate counterclockwise from the origin (TR′ > TR). Although the total cost curve may also increase as state rent seeking stimulates higher transactions costs of revenue collection, the curve must rise at a lagging rate. Higher state rents do not increase the costs of producing protection from external threats per se, and this is likely to be the major component of the cost curve. And even if the transactions costs of revenue collection increase, because important social groups demand compensation or public unrest must be suppressed, collective action problems thwart a fully countervailing societal response. Assuming for expositional clarity that total costs remain constant, state rent seeking raises total revenue and expands the optimal size of the political unit from *O* to *O′*. Intuitively, with rents, each unit of territory produces greater revenue for the state. The greater the revenue, the greater the equilibrium costs that can be borne to capture that revenue. Thus, a state with an increased rent-seeking capacity has an incentive to expand until marginal revenue and mar-

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ginal cost are once again equalized at a new, larger size.

While this expansion (from O to O') benefits the state, it harms society. With TR′ representing the revenue of a successful rent-seeking state, the line segment b′c′ will be the state's economic profit and a′b′ its rent, with a′b′ being redistributed away from society to state officials. Not only is society exploited by state rent seeking, but it is doubly hurt by the additional expansion rent seeking induces. In the absence of expansion, the state's rents would be ab; with expansion, these rents increase to a′b′. Simple observation suffices to show that a′b′ will always be longer than ab.

Both the original citizens or consumers of state-produced services and the individuals newly incorporated into the territorial unit pay the new higher, expansion-induced price for protection. In other words, greater rents are extracted from both the original and augmented populations, although the exact rate at which these groups will be taxed is determined by their relative costs of controlling the state. The previously foreign population does not bear the burden of the higher rents alone; all consumers in that particular state face a higher price for protection. Imperialism is not simply a means for extracting wealth from "foreign" territories: it is a tool used by the state for exploiting its own society as well.

While this argument has been developed only in terms of a single state service (protection), any rents earned on any service provided by the state increase the optimal size of the political unit. Even if rents are earned from providing, say, the physical infrastructure, the net benefits to the state of providing that good still increase and provide an incentive for further expansion. Fully generalized to all areas of state service, the state optimizes where the marginal revenue (including rents) equals the marginal costs of providing all services and collecting all revenues. Given that the demand for protection is likely to be more inelastic than the demand for other state services, however, the highest rents are likely to be earned in this industry. Nor is there any apparent reason why societal constraints on the state should differ significantly across areas of service.[16] Hence, it is appropriate to focus on the production of protection. Nonetheless, it is important to note that in a fully generalized theoretical framework, any rents earned by the state are sufficient to generate an imperialist bias.

It is virtually impossible to measure state rents directly. Perhaps in past centuries, when the public fisc and the private purse of the ruler were one and the same, it might have been possible to observe the rents earned by the state by measuring the comparative opulence of the royal court. If the general argument developed here is correct, however, then surely one of the primary tasks of modern state budgeting techniques is to obscure the difference between normal and supernormal state profits. It also follows that in the countries where they are most easily observed, rents will be relatively low—even zero, at the extreme.[17] Given these measurement difficulties, no direct test of the theory is possible. Rather, it can be assessed—and the presence of state rents revealed—only by examining the theory's behavioral implications. Accordingly, I shall examine several hypotheses derived from the theory. In doing so, I also provide an explanation for one of the longest-standing puzzles of international relations.

THE PROPENSITY FOR WAR

In a recent review of the literature, Levy writes that (1) "the evidence shows that the proportional frequency of war involvement of democratic states has not been greater than that for nondemocratic states"; (2) "democracies may be slightly less likely than nondemocratic states to initiate wars, but the evidence is not yet conclusive on this question"; and (3) "although democracies have fought wars as frequently as have nondemocratic states, they almost never fight each other. . . . *This absence of war between democratic states comes as close as anything we have to an empirical law in international relations*" (1989, 270; my emphasis). In Russett's view, this final result "is one of the strongest nontrivial or nontautological generalizations that can be made about international relations" (1990, 123).[18]

For illustration, all interstate wars from 1816 to 1988 involving democratic states are listed in the Appendix.[19] Of these 30 conflicts, only 2 involve democratic states fighting each other: World War II, where Finland fought alongside the Axis (an easily explained exception), and the Spanish–American War of 1898.

This pattern of pacifism only among democratic states is inconsistent with most prevailing theories of international politics (Doyle 1983a, 218–25; Levy 1989, 270). Realism, which focuses on the *universal* effects of international anarchy, the security dilemma, and the balance of power, cannot account for the relative pacifism of democracies. This is even more true of its contemporary variant, neorealism, which explicitly abstracts from the domestic characteristics of nation–states. Given the correlation between democratic political structures and capitalism, Marxist–Leninist theories predict a higher incidence of war between democracies. Conversely, liberal economic theory, which highlights the pacifying effects of commerce, predicts a lower *overall* incidence of democratic war involvement, not just a lower probability of war among democracies.

The most persuasive account of the relative pacifism of democracies was first put forth by Immanuel Kant in 1795, when there were less than a handful of democracies in existence, and has been recently summarized and extended by Doyle (1983a, 1983b, 1986; see also Russett 1990, 124–32). There are three steps in Kant's argument. He first posits that republican forms of government, the inevitable result of political evolution, will replace monarchical (or autocratic)

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caprice with populist caution. Kant "argues that once the aggressive interests of absolutist monarchies are tamed and once the habit of respect for individual rights is engrained by republican government, wars would appear as the disaster to the people's welfare than he and other liberals thought them to be" (Doyle 1983a, 229).

While republican rule guarantees caution, international law guarantees mutual respect among liberal states—a step that separates Kant from other liberal thinkers:

> As republics emerge . . . and as culture progresses, an understanding of the legitimate rights of all citizens and of all republics comes into play; and this, now that caution characterizes policy, sets up the *moral foundations for the liberal peace*. . . . In short, domestically just republics, which rest on consent, presume foreign republics to be also consensual, just, and therefore deserving of accommodation" (Doyle 1983a, 230; my emphasis).[20]

Finally, drawing upon liberal economic theory, Kant concludes that cosmopolitan law, especially the "spirit of commerce," provides a material incentive for states to promote peace and avert war—a third step that reinforces the basis for mutual respect.

Doyle (1983b) extends Kant by identifying and examining the moral imperative that liberalism creates for democracies in their relations with nondemocratic states.[21] "If the legitimacy of state action rests on the fact that it respects and effectively represents morally autonomous individuals," he argues, "then states that coerce their citizens or foreign residents lack moral legitimacy" (p. 325). It follows, then, that "the liberal dictum in favor of nonintervention does not hold. Respecting a nonliberal state's state rights to noninterference requires ignoring the violations of rights they inflict on their own populations" (p. 330). Thus, the moral foundations of the liberal peace are absent in relations with autocratic states, allowing war to be used as an instrument of statecraft and, at the extreme, necessitating active intervention by liberal states in the internal affairs of autocracies.

Kant's theory and Doyle's extension constitute a normative philosophy for the conduct of foreign affairs but not a positive theory of international relations. This philosophy rests, fundamentally, upon a moral imperative of *restraint*, in which democratic states must forgo potentially welfare-improving actions that would be damaging to their liberal brethren, and *necessary action*, in which a democracy might actually reduce its own material welfare in order to overthrow an autocracy and free a repressed people.[22] While it forms a sophisticated and coherent worldview, liberalism nonetheless contains a curious combination of motivations—a combination that can be reconciled only if we assume that individuals are in fact essentially moral actors willing to forgo material gains for normative ends. But if we accept this assumption, then the exploitative behavior of autocratic rulers is theoretically unmotivated: if individuals are moral and will act morally given the opportunity, why should autocrats, who are unconstrained by society, act immorally? And if power corrupts the ruler, why should collectivities be immune—especially in their relations with other states? Kant's suggestion that autocratic rulers might "resolve on war as a pleasure party for the most trivial reasons" (quoted in Doyle 1983a, 229) does not resolve this contradiction. Kant's liberalism ultimately falls short of being a positive *theory* of democratic pacifism because it lacks what today we would call a fully developed and consistent set of micro motives.[23]

The theory I have summarized above offers an alternative explanation for the relative pacifism of democratic states that nonetheless builds upon, and subsumes, many of Kant's essential insights. First, as I have demonstrated, democratic states will tend to be less expansionist than autocratic states. The larger the rent-earning ability of the state, the greater the optimal size of the territorial unit, and the greater the incentives for the state to try to reach this optimal size. To the extent that war is a necessary byproduct of expansion, it follows (ceteris paribus) that autocratic states will be more war-prone. Where, for Kant, republican institutions restrain the capriciousness of the ruler, democracy, in this approach, constrains the ability of the state to extract monopoly rents at society's expense.

Second, the theory suggests that democracies will often be the object of expansion by autocratic states. Democracies pose two threats to the rent-seeking ability of autocracies. As I have noted, democracies, by their very existence, serve as magnets that pull individuals out of autocratic polities; in the absence of a low-rent democratic haven, exit is less likely to occur. By eliminating democracies, autocratic states can reduce the gains from, and incentives for, emigration. In addition, through their political openness and richer informational environments, democratic states reduce the costs to autocratic societies of monitoring state behavior; by observing democracies, citizens in autocracies are more likely to become aware of the magnitude and consequences of state rent seeking.[24] Again, by eliminating democracies abroad, autocratic states solidify and reinforce their rent-earning abilities. For both of these reasons, democracies, perhaps unwittingly, may become targets of the expansionary activity of autocracies—thus expanding their overall war involvement despite their own pacific nature.[25]

Finally, democratic states may engage in expansion and even intervene in other countries, as Doyle claims, but only under restricted conditions. Democracies will expand *only* when the initial costs of conquest and ongoing costs of rule are less than the discounted present value of future economic profits. Under these conditions, expansion is socially optimal regardless of regime type. Nineteenth-century European imperialism may provide examples of such socially efficient expansion: in many areas of the periphery the cost of war against more "primitive" peoples was relatively low and the potential gain from the erection of a "modern" state with its greater extractive capacity disproportionately large. With the rise of "European-style" states in the periphery dur-

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ing the twentieth century, however, the cost of imperialist expansion has substantially increased, apparently foreclosing this option.

By the same principle, democracies may also preemptively intervene in the domestic affairs of an autocracy to construct democratic political structures as long as the costs of the intervention are less than the expected costs of a war stimulated by state rent seeking. In this view, the proactive policy of democracies rests not on a moral imperative but on a rational calculus of preemption.

Together, these propositions imply that democracies are, on average, no more or less war prone than other states. On the other hand, democracies are less likely to fight each other, for only in this area is the absence of an imperialist bias manifest. Indeed, the almost complete lack of war between democracies suggests just how important state rent seeking may be as a source of international conflict.

THE PROPENSITY FOR VICTORY

Democracies are not only less likely to wage war with each other, they are also significantly more likely to win the wars they fight against autocracies. Liberalism offers a possible explanation for this correlation: democracies tend to turn international conflicts into ideological crusades and demand total victory.[26] Yet, as I have noted, the micro motives for democratic crusading are, at best, unclear. Moreover, within this framework, ideological fervor need not translate into victory; for the fear that the winner will transform the loser's social structure may only spur the failing side onto greater efforts. The drive for victory need not yield the desired result.

The theory summarized in Part I suggests a second explanation. To the extent that democratic states earn fewer rents, it follows that they tend (1) to create fewer economic distortions, possess greater national wealth, and devote more resources to security; (2) to enjoy greater societal support for their policies and therefore a greater extractive capacity; and (3) to form overwhelming countercoalitions against expansionist autocracies. I shall develop each of these propositions in turn.

State rents, like the rents earned by private actors, distort patterns of production and consumption, divert resources into directly unproductive activity, create social deadweight losses, and thereby reduce the total product of goods and services within an economy. While the successful rent earner is better off, the economy as a whole suffers. At the very least, growth rates lag behind their potential. North (1981) has examined the effects of state rent seeking on property rights and, in turn, growth; but the argument is more general.[27] Any state rents, including monopoly rents earned through the exchange of state-produced services such as protection, distort the economy and, over time, lower national wealth relative to its potential.[28]

The wealthier the country (ceteris paribus), the more absolute resources it devotes to producing security. For any given set of costs and preferences, wealthier countries produce and consume greater amounts of all goods and services, including protection; under normal conditions, an increase in wealth affects only the level, not the mix, of goods in the economy. It follows, then, that wealthier countries produce and consume greater amounts of security (i.e., provide greater absolute resources to the state for producing protection). In contests where sheer resources matter, the cumulative effects of lower state rents may prove decisive.

Democratic states should also possess greater societal support for their policies, suggesting that they will enjoy a greater extractive capacity for any given level of national wealth. All societies make trade-offs between consuming what are mostly private goods and services and consuming state-produced protection; while both constitute current consumption, the latter serves as insurance that societies will be able to enjoy their present and future holdings of wealth. During hostilities, when the external threat to national wealth, territorial integrity, and the present form of rule is most acute, individuals and, in turn, society will tend to purchase greater quantities of protection (pay higher taxes), thereby transferring greater resources to the state for the waging of war.

In view of their exploitative nature, however, autocratic states pose a greater threat than do nonautocratic states to the current and future ability of democratic societies to produce and consume wealth; as a result, democracies should demand greater protection against these threats and contribute proportionately more to ensure victory in war. Within the theory developed here, individuals are indifferent between being ruled by two equally democratic regimes; each provides similar levels of protection close to the normal profit price (p in Figure 1). As a result, there is no incentive for citizens of one democracy to purchase protection against another equally democratic state unless there is some uncertainty about the likely behavior of the democratic conqueror. Autocratic states, on the other hand, provide protection only at a higher price, with the difference between the normal profit and higher prices (p and r in Figure 1) being captured by the state as rents. In this case, citizens of democracies do possess incentives to purchase protection against this threat to their current and future wealth; indeed, the greater the expected rent seeking from an autocratic conqueror, the more protection citizens will demand, and pay for, from their present democratic state.

Conversely, autocratic societies may actually benefit from defeat—if the victorious democratic states remake the autocracies in their own images. As a result, societies in autocratic polities should be willing to contribute proportionately *less* than democratic societies. While autocratic states will seek to offset this tendency by vilifying the enemy (instilling fear that defeat will mean national destruction, rape, and slavery), a fifth column remains a real possibility. Autocracies may also increase the degree of coercion

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used to extract resources; but this implies higher transactions costs for revenue collection, which will further disadvantage them relative to democracies. At the very least, the lack of societal support places a real constraint on their extractive capabilities. Lamborn's (1983, 1991) study of the great powers during the late nineteenth century supports this expectation, as does the tendency of states to expand the franchise or otherwise liberalize politically during or immediately after major wars.[29]

Finally, to the extent that states balance threats, rather than power (as Walt [1987] has argued and as is consistent with the logic I have developed), democratic states should form overwhelming countercoalitions against autocratic states. Not only are autocracies more likely to seek territorial expansion, they are more likely to target democracies (to reduce exit options). In addition, autocratic expansion poses a greater threat to democracies because of the larger rents the state is likely to extract if successful. The greater the threat (ceteris paribus), the greater the balancing reaction by other states.

Given its imperialist bias and likely behavior if successful, the threat posed by an autocracy is proportionately greater than the sum of its aggregate resources, the traditional measure of national power. It follows that the countercoalition that forms against any autocracy should be disproportionately large or overwhelming. If autocracies have greater incentives to expand, democracies have greater incentives to resist. As a result, this coalition should also be disproportionately composed of democractic states.[30] The overlarge "democratic" coalition should *deter* autocratic expansion (by raising the costs of conquest) and, if deterrence fails, be more likely to win. Combined with the greater wealth and extractive capabilities of the individual states, this suggests that the democratic coalition should be virtually invincible. The overlarge coalitions formed during World Wars I and II and, more strikingly, the Cold War (often understood as anomalies in realist theories of international relations; see Waltz 1979) bear out this expectation.

Evidence

Many of the concepts central to the theory summarized here are difficult to operationalize or lie beyond current data-gathering techniques. Extractive capability, for example, is unmeasurable: military spending as a proportion of gross national product (a commonly used indicator) may be distorted and inflated by state rents. Nonetheless, the propositions I have developed suggest that democratic states should tend to win wars—a derived hypothesis that provides an indirect test of the theory.

The historical record is striking. Of the 30 wars listed in the Appendix, 3 (the Korean, Israeli–Egyptian, and Israeli–Syrian Wars) must be excluded from this analysis for want of a clear victor. I also exclude the Spanish–American War, fought between two democracies, but include World War II despite Finland's exceptional position. Of the 26 wars fought since 1816 between democracies and autocracies, the former have won 21 (81%) and lost 5 (19%). In other words, democratic states, either singly or in combination with other states, have won four times as many wars as autocratic states. Excluding the First and Second Balkan Wars, where Greece was the sole democracy on the winning side, does not appreciably change these results: the democracies still win 19 (79%) of 24 wars.

TABLE 1

Regime Type and Victory in War (Individual Participants)

SUCCESS IN WAR	NATURE OF REGIME: AUTOCRATIC	DEMOCRATIC	TOTAL
Loser	42	9	51
Winner	32	38	70
Total	74	47	121

Note: All wars in the Appendix are included except the Spanish–American War of 1898 and wars in which no clear winner emerged. Gamma = .694; chi squared = 16.673; df = 1; p = .000046.

Scoring each participant individually yields a strong and significant correlation between democratic victory and autocratic defeat. Table 1 breaks down all 121 participants in the 26 wars according to regime type and outcome. This construction biases the results against the hypothesis by coding as winners the not-inconsequential number of autocracies who fought as members of victorious democratic coalitions. Nonetheless, the degree of association is strong, indicating that even with this bias democratic states are significantly more likely to win—and autocratic states more likely to lose—than the converse.

Rather than relying upon a simple dichotomy, it is possible to examine the average degree of democracy in the sets of winners and losers of these 26 wars. Using an 11-point scale of democracy (0–10), the mean of the 70 winners is 5.60 and the mean of the 51 losers is 2.55.[31] The probability that these figures would emerge by chance is less than .001 (t = 4.43; df = 119).

Finally, the relationship between democracy and victory is quite robust. Table 2 presents a logit analysis performed with the 121 war participants as the units of observation. Along with democracy, the analysis included military personnel, a measure of military strength, iron and steel production, a proxy for industrial capacity, and a dummy variable indicating whether or not the country initiated the war.[32] Common sense and, for war initiation, previous research (Bueno de Mesquita 1981, 22), suggest that all of these relationships should be positively related to victory.

Democracy is consistently positive and significant, offering strong support for the argument developed here. Military personnel and iron and steel production, on the other hand, are insignificant at standard

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TABLE 2

Logit Analysis of Victory in War

VARIABLE	EQUATION 1	EQUATION 2
Constant	.4214	−.7527*
	(.4461)	(.3346)
Democracy (0–10)	.1933*	.2524***
	(.0763)	(.0597)
Military personnel (millions)	.3701	.4968
	(.5325)	(.4188)
Iron and steel production (millions of tons)	.0024	−.0334
	(.0542)	(.0201)
Initiator[a]	−1.3982**	—
	(.5126)	
Log likelihood	−48.05	−71.63
n	87	121
Percent correctly predicted	72.41	67.77

Note: Standard errors are in parentheses. Dependent variable is war outcome (loser = 0; winner = 1).
[a]0 = *no*; 1 = *yes*.
*$p < .02$.
**$p < .01$.
***$p < .001$.

levels in both equations, suggesting that in this set of wars neither military nor economic strength is associated with victory. When included, war initiation is significant but in the wrong direction; surprisingly, in wars between democracies and autocracies, noninitiators win more frequently.

The failure of these alternative explanations to predict victory or loss correctly does not imply that common sense or previous research is wrong. Rather, it highlights the exceptional nature of war between democracies and autocracies. In these conflicts, military strength, industrial capacity, and the ability to choose to wage war appear to be far less important determinants of victory than governmental form.

CONCLUSION

Regime type does matter in international politics. Democracies are less likely to fight wars with each other. They are also more likely to prevail in wars with autocratic states. This syndrome of powerful pacifism accords, in part, with "Kantian" liberalism; but because of inconsistent behavioral assumptions, this normative frame cannot be said to constitute a positive theory of international relations.

I have offered an alternative explanation drawn from the microeconomic theory of the state. Specifically, state rent-seeking creates an imperialist bias in a country's foreign policy. This bias is smallest in democracies, where the costs to society of controlling the state are relatively low, and greatest in autocracies, where the costs are higher. As a result, autocracies will be more expansionist and, in turn, war prone. To the extent that democracies do wage occasional wars of expansion, intervene in the domestic affairs of autocracies, and are targets of autocratic expansion, there should be no significant overall difference in their frequency of war involvement. Only in their relations with each other does the relative pacifism of democracies appear. In addition, democracies (constrained by their societies from earning rents) create fewer economic distortions and possess greater national wealth, enjoy greater societal support for their policies, and tend to form overwhelming counter-coalitions against expansionist autocracies. Thus, democracies will be more likely to win wars.

If democracies are powerful pacifists, why do autocracies persist within the international system? It follows from the arguments I have outlined that democracy is an evolutionarily superior and stable form of rule. If so, why has democracy not displaced autocracy?

On the one hand, democracy *has* expanded. The number of democratic countries has grown from a mere handful in the eighteenth century to over 60 in the early 1980s.[33] The recent transformations in Eastern Europe suggest the promise of further liberalizations elsewhere, although in my opinion the tide could easily be reversed. But given the relative infrequency of war, there is no reason to presume that political change will be rapid; the evolutionary pace of the global system may well be glacial.

On the other hand, democracies only *tend* to win wars; historically, for every four they win, they lose one. Nor is there any consistent trend in favor of the democracies: autocracies were most successful in the 1960s, winning half of the wars in which they were engaged, but entirely unsuccessful in the 1970s and 1980s (see Appendix). Any evolution toward greater democracy is likely to be characterized by fits and starts. Four steps forward, one step back.

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APPENDIX: ALL WARS INVOLVING DEMOCRATIC STATES, 1816–1988

In the following list, boldface in the righthand column indicate democratic states. Bulgaria, Romania, Italy/Sardinia, and France which fought on both sides of World War II are listed here on their initial sides.

WAR	PARTICIPANTS (WINNERS FIRST)
1. Mexican–American 1846–48	**United States** vs. Mexico
2. Roman Republic 1849	The two Sicilies, **France**, and Austria–Hungary vs. the Papal States
3. Crimean 1853–56	**United Kingdom**, Italy/Sardinia, France, and Turkey/Ottoman Empire vs. USSR/Russia
4. Anglo–Persian 1856–57	**United Kingdom** vs. Iran/Persia
5. Sino–French 1884–85	**France** vs. China
6. Greco–Turkish 1897	Turkey/Ottoman Empire vs. **Greece**
7. Spanish–American 1898	**United States** vs. **Spain**
8. Boxer Rebellion 1900	Japan, **United Kingdom**, USSR/Russia, **France** and **United States** vs. China
9. Spanish–Moroccan 1909–10	**Spain** vs. Morocco
10. First Balkan 1912–13	**Greece**, Yugoslavia/Serbia, and Bulgaria vs. Turkey/Ottoman Empire
11. Second Balkan 1913	Turkey/Ottoman Empire, **Greece**, Yugoslavia/Serbia, and Romania vs. Bulgaria
12. First World War 1914–18	Japan, Belgium, **United States**, Yugloslavia/Serbia, **United Kingdom**, **Portugal**, Romania, **France**, **Greece**, Italy/Sardinia, and USSR/Russia vs. Turkey/Ottoman Empire, Austria–Hungary, Germany/Prussia, and Bulgaria
13. Hungarian–Allies 1919	Romania and **Czechoslovakia** vs. Hungary
14. Russo–Polish 1919–20	**Poland** vs. USSR/Russia
15. Russo–Finnish 1939–40	USSR/Russia vs. **Finland**
16. Second World War 1939–45	**United Kingdom**, **Australia**, **Canada**, Ethiopia, Poland, **United States**, USSR/Russia, **Belgium**, Brazil, China, Yugoslavia/Serbia, **Netherlands**, **New Zealand**, **France**, **South Africa**, Greece, **Norway**, and Mongolia, vs. Japan, Italy/Sardinia, Germany/Prussia, Romania, Bulgaria, Hungary, and **Finland**
17. Palestine 1948	**Israel** vs. Jordan, Iraq, Syria, Egypt/UAR, and Lebanon
18. Korean 1950–53 (no clear victor)	**Greece**, **United Kingdom**, **Canada**, Thailand, **Belgium**, **Turkey/Ottoman Empire**, **United States**, **Australia**, **Netherlands**, Ethiopia, Columbia, **Philippines**, **France**, and South Korea vs. North Korea and China
19. Sinai 1956	**United Kingdom**, **France**, and **Israel** vs. Egypt/UAR
20. Sino–Indian 1962	China vs. **India**
21. Second Kashmir 1965	Pakistan vs. **India**
22. Vietnamese 1965–75	North Vietnam vs. Thailand, South Vietnam, **United States**, Kampuchea/Cambodia, **Republic of Korea**, **Australia**, and **Philippines**
23. Six Day 1967	**Israel** vs. Egypt/UAR, Jordan, and Syria
24. Football 1969	**El Salvador** vs. Honduras
25. Israeli–Egyptian 1969–70 (no clear victor)	**Israel** vs. Egypt/UAR
26. Bangladesh 1971	**India** vs. Pakistan
27. Yom Kippur 1973	**Israel** vs. Egypt/UAR, Iraq, Syria, Jordan, and Saudi Arabia
28. Turko–Cypriot 1974	**Turkey/Ottoman Empire** vs. Cyprus
29. Falklands 1982	**United Kingdom** vs. Argentina
30. Israeli–Syrian (Lebanon) 1982 (no clear victor)	**Israel** vs. Syria

Notes

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1. For an extreme view, see Crozier, Huntington, and Watanuki (1975). Russett argues that cyclical majorities may be the greatest problem facing the democratic formulation of foreign policy (1990, 115–7).

2. This theory has also been referred to as the neoclassical theory of the state (North 1981) and the predatory theory (Levi 1983, 1988). Both of these appellations have misleading

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normative implications. I prefer the more neutral term used here. See also Auster and Silver 1979. Within this approach there is some disagreement about the objective that states pursue. North and Levi, for instance, both assume that states maximize revenue, while I assume that states seek profits. For an elaboration of this debate and defense of the profit-maximizing assumption, see Lake 1990.

3. In producing protection and collecting revenues, the state consumes resources—everything from paper clips and stationary to stealth bombers and nuclear weapons. States also consume labor. As noted, the real cost of these resources is determined by their fair market value or (in cases where the state is a dominant consumer) the price the state could obtain if it exercised its monopsonist power—a factor much more important for weapons than, say, for office supplies.

4. The public nature of the good protection complicates the definition of price and demand. By price, I mean the rate at which the state extracts wealth (for practical purposes, the tax rate [t] multiplied by the inverse of the probability (p) of successful evasion (free riding); that is, price = $t(1 - p)$. By *positive demand* I mean that individuals prefer some level of protection at some positive cost to themselves (i.e., price) to no protection at zero cost. Strictly speaking, the demand for public goods is summarized in an evaluation schedule, which differs from a traditional demand curve only in that individual preferences are added vertically, rather than horizontally, in order to reflect nonrivalry in consumption. Following common practice, I nonetheless refer to this evaluation schedule as a demand curve. On the treatment of public goods within a Marshallian economic framework, see the seminal work by Buchanan (1968). My approach differs from that of Buchanan (who adopts an essentially pluralist approach to politics) by including a theory of the state as a monopoly provider of public goods. This has important implications for the point of equilibrium production. See n. 6.

5. Plotting the probability of success in armed conflict against the quantity of resources mobilized for war, Emmerson (1983, 432) argues that protection forms an S-shaped curve and that once the probability of success reaches 1.0, the country has entered a "zone of redundancy" in which it has "overmobilized." This clearly implies that the demand for protection can be sated. As I have argued however, the security dilemma (and the action–reaction syndrome implied therein) is likely to prevent the probability of success from reaching its upper bounds. Emmerson further argues that charismatic leaders are most likely to arise when the probability of success is in its middling range and that once a state enters the zone of redundancy, it and its leadership will decay. Interestingly, Emmerson's thesis is contradicted by his own evidence. The ancient states of Balti (the focus of his essay) began to decline in 1846 when they were absorbed by the maharaja of Kashmir and, behind him, the British Government of India (p. 438). This absorption, of course, implies that the Balti's probability of military success against these superior powers was significantly less than 1.0.

6. In pure competition, the firm also produces where marginal cost (MC) equals marginal revenue (MR). Facing a horizontal demand curve, however, marginal revenue equals price and, in turn, demand (D); and the competitive firm, in theory, produces where marginal cost equals demand, that is, where MR = D = MC = MR. In Buchanan's (1968) essentially pluralist view, this is also the Pareto optimal equilibrium for the production of public goods and the point against which all distortions must be measured. Adding a concept of the state as a monopoly provider of public goods significantly alters this equilibrium, however. With a downward-sloping monopoly firm demand curve, supplying protection where marginal cost equals demand would result in a significantly larger quantity; but all units to the right of q in Figure 1 would be produced at a loss (MC > MR). What is efficient for society is inefficient for the state, and vice versa. I shall argue that society can control, in part, the quantity of monopoly rents earned by the state; likewise, society can control in part the level of production, with the resulting equilibrium arising somewhere between MR = MC (the state's preferred level) and MC = D (society's preferred level). For expositional clarity, I shall assume henceforth that production occurs where MR = MC and that political conflict occurs primarily over the level of monopoly rents earned by the state. Relaxing this assumption, however, only reinforces my arguments on democracy and the syndrome of powerful pacifism.

7. At the minimum, the protection service must generate normal profits for state officials, defined as the reservation wage; if the state fails to earn at least normal profits, the individuals who comprise it will leave and assume other employment.

8. This conclusion follows even without the assumption advanced in n. 6 as long as society faces positive costs for monitoring state behavior and engaging in exit and voice.

9. On principal–agent problems, see Fama 1980; Fama and Jensen 1983a, 1983b; Jensen and Meckling 1976; and Ross 1973.

10. A free press is an exception to this rule. News organizations specialize in collecting information (especially on state activities) and selling it to the public.

11. See Tiebout 1956. This is analogous to North's (1981) concept of external competition, where other states exist to act as alternative suppliers of protection. On asset mobility, see Bates and Lien 1985. The phrase *low rent* refers not to the overall tax rate but only to the level of supernormal profits earned by the state on the services it provides. Different states are likely to provide different mixes of services with corresponding tax levies. Following Tiebout, emigrants are likely to select the polity that offers the mix of services they want at the price they are willing to pay. The analysis here focuses only on state rents, which distort the implications of the now-standard Tiebout analysis.

12. Although the direct costs for voting are small to nil, there are certainly opportunity costs for activities forgone and possibly some indirect costs (e.g., transportation to the polling station).

13. This is not to deny that some groups within autocratic polities can participate at relatively low cost and thus have significant influence over the state. In these circumstances, it is expected that the powerful social groups would share in the rents earned by the state. The argument here hinges on the average cost for society as a whole. See also n. 15.

14. Expansionary policy is to be taken in opposition to unilateralism and cooperation. See Lake 1990, 1991. This analysis contrasts with other explanations of so-called overexpansion or with a country's expansion beyond the point where the marginal costs equal the marginal gains to society. Rather than being driven by perverse incentives within the international system, cognitive bias, dominant social groups, or log rolls between concentrated social interests (see Snyder 1991), the explanation developed here focuses on the monopoly structure of the protection industry and the costs to society of regulating state behavior.

15. This argument applies only for rents earned and retained by the state. If the state is merely a conduit for redistributing wealth between social groups, no imperialist bias will emerge. Thus, I differ crucially from much of the extant literature on rent seeking, which tends to focus on the actions of *social* groups (e.g., Olson 1982). While social rent seeking will reduce national wealth as well, I have yet to see a convincing argument that democracies are more prone to social rent seeking than autocracies. Interestingly, even though Olson adopts a pluralist conception of the state, he argues that his theory is supported by evidence from both developed democracies and nondemocratic, non-Westernized polities (1982, 146–80). Although I do not find it persuasive, Ekelund and Tollison (1981) do argue that democracies will actually experience less societal rent seeking than autocracies. My intuition is that democratic and autocratic societies extract similar levels of rents but distribute them over greater and smaller sets of groups, respectively. I contend here, however, that rents earned by democratic *states* are significantly less than the rents extracted by autocratic *states*.

16. Monitoring costs will be higher, however, where the

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state can persuade society that "national security" considerations require a higher degree of secrecy.

17. Just as the absence of supernormal profits by entrepreneurs in perfectly competitive economic markets does not vitiate the assumption of profit maximization and, in fact, follows from this assumption, the absence of rents does not undermine the assumption of state profit maximization. Rather, the receipt of only normal profits in ordinary economic enterprises and the state reflects the presence of acute competition and the constraints of society, respectively.

18. For studies supporting these conclusions, see Chan 1984; Ember, Ember, and Russett 1991; Maoz and Abdolali 1989; Rummel 1983; Small and Singer 1976; and Weede 1984.

19. Wars and participants are from Small and Singer 1982, updated through 1988 from Singer 1991. Regime type is from Gurr 1990. Democracy (DEMOC, variable 6.2) is an additive 11-point scale (0–10) based on the competitiveness of political participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. Countries that score 6 or more on this scale are classified here as democracies; countries that score 5 or less, as autocracies. There are relatively few countries that score in the middle of this range. Although countries that score 5 on this scale are not, in my view, normally considered to be democratic, lowering the threshold a point or two would not significantly alter this list of wars or the analysis.

20. In a personal communication, Michael Doyle has suggested that it is this mutual republican recognition that separates modern democracies from the ancient Greek democracies, which appear not to have been substantially more pacific than their autocratic contemporaries (see Russett and Antholis 1991). My explanation would focus on the relatively narrow franchise in these so-called democracies.

21. Doyle distinguishes between strong and weak autocrats, suggesting that democracies will not war with the former (a point consistent with my analysis below) but will fail to take advantage of mutually advantageous agreements (such as arms control) or opportunities to exploit such states. I question the extent to which this "stylized fact" holds, as democracies often do ally with one autocrat against another even in times of peace. The United States, for instance, played the "China card" in the 1970s. Nonetheless, to the extent that this tendency does exist, it cannot be reconciled with the theory summarized here.

22. Although only implied in their writings, this emphasis on forgoing material gains and incurring material losses is essential for both Kant and Doyle; in its absence, democratic pacifism is not moral but is economically efficient—a position that both authors claim to supersede.

23. Several scholars have recently attempted to formalize a model of foreign policy decisionmaking that focuses on the greater domestic constraints democratic societies exert on their leaders. These "neo-Kantians," however, suffer from the same problem as their intellectual progenitor. Morgan and Campbell base their analysis on "two key assumptions: (1) that a democratic political structure imposes constraints on the decision-making process by restricting the key decision makers' freedom of choice; and (2) that with regard to decisions for war, these constraints push toward peace (i.e., heads of state would be more likely to opt for war than those they govern) (1991, 189–90)." Bueno de Mesquita and Lalman (1992) find empirical support for Morgan and Campbell's first assertion but do not examine the second, more fundamental proposition. Like Kant, these authors do not deduce motivations or explain why leaders are more war-prone and the populace more pacific. While it might be understood as an alternative explanation of democratic pacifism, I see this work as complementary to the analysis offered here.

24. This suggests why "insulation" is an essential element of the totalitarian model. See Friedrich, Curtis, and Benjamin 1969.

25. This argument also implies that autocracies should initiate wars with democracies. Given the complexities of military strategy and the often-considerable gains from surpise attack and preemption, however, who actually fires the first shot or crosses the border (the basis of most standardized codings of war "initiation") is a poor indicator of who actually provoked, or "caused," the war. In addition, since history is typically written by the victors (and, as I shall show, democracies win a disproportionate number of wars), even more subjective estimates of war initiation may well be biased. As a result, I do not place much faith in patterns of war initiation as a test of the theory presented here. Following the coding rules used in Table 1, however, the correlation (gamma) between democracy and war initiation is correctly signed but very weak (−.04) and insignificant. To the extent that this result is valid, it must count against the theory.

26. Contradicting the "crusading" hypothesis, Russett finds that the surge in public support for the president following an aggressive foreign policy action (often referred to as the "rally-round-the-flag" phenomenon) is typically short-lived, with a half-life of no more than a month or two (1990, 94).

27. See especially Olson 1991, which extends to the state Olson's earlier work on social coalitions. Echoing North and the analysis here, he finds that democracies should experience lower state rents and higher growth.

28. This is a long-run, dynamic argument; it does not imply that all democracies are wealthier than all autocracies. The entire literature on the microeconomic theory of the state directly contradicts that on the so-called developmental state. The latter approach asserts that strong, autonomous states have to deter social rent seeking and stimulate growth under conditions of late industrialization (see, e.g., Evans 1979). As I suggest in n. 15, I do not expect there to be a systematic difference between the *level* of rents extracted by social groups in democracies and autocracies. Moreover, the literature on the developmental state is generally mute on the motivations of state officials: Why do political leaders choose the "public" interest over their own private interests? The microeconomic theory of the state suggests that strong, autonomous states will typically exploit, rather than develop, their societies except where they face what North (1981) has termed severe *external constraints* (analogous here to very low costs of exit). Interestingly, most, but not all, of the developmental states have experienced clear threats from other states (e.g., South and North Korea, Taiwan and China). In such polities, especially those producing far within their production possibility frontiers, this analysis predicts that states will opt for increased normal profits from growth over increased rents from redistribution. In the long run, the theory suggests that these developmental autocracies will either ossify and once again become a drag on development (as in the former Soviet Union) or liberalize (as in South Korea).

29. For the domestic consequences of war, see Stein 1978. On political reform in Germany during World War I, see Feldman 1966; and on the Soviet Union during World War II, see McCagg 1978, esp. 18–23.

30. Siverson and Emmons (1991) find that democracies have a greater propensity to ally with one another than with other states.

31. See n. 19 above. As noted, at least one participant in each war is democratic (defined by a minimum score of 6 on an 11-point scale). The mean score of 5.60 suggests that the "average" winner in these 26 wars was autocratic, although there were more democracies (38) than autocracies (32) in the population (see Table 1).

32. Data on military personnel and iron and steel production are from the July 1990 Correlates of War, National Materials Capabilities data set (J. David Singer and Melvin Small, principal investigators). An attempt was made to include data on military expenditures and energy usage from this same data set, but there were too many missing values for the early nineteenth century. Data on war initiation is from Small and Singer 1976. Unfortunately, this set covers only the period through 1965 and does not include several wars later added in Small and Singer 1982; this accounts for the reduction in the number of observations in Table 2, equation 2. The proviso stated in n. 25 holds here, as well. I also assessed the

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possible presence of interaction effects among these measures by multiplying democracy with military personnel, iron and steel production, and war initiation. In every case, these interaction terms were insignificant. These results are not reported here.

33. On the diffusion of democracy, see Starr 1991.

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POLITICS AND THE STRUCTURAL DEPENDENCE OF THE STATE IN DEMOCRATIC CAPITALIST NATIONS

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I explore empirically a central claim of the structural dependence thesis, namely, that capitalists' ability to disinvest fundamentally conditions policy choices in democratic capitalist systems. Utilizing time-series data for 16 affluent democracies from 1965 to 1984, I find that, indeed, low rates of business investment are associated with reductions in corporate tax burdens and that these reductions are more pronounced in periods of economic crisis. Moreover, low rates of capital formation engender cuts in personal income taxes during periods of economic stress. However, I also find that the magnitude of responsiveness of taxation to low rates of investment is relatively small and that analyses of the political context of investment and taxation indicate that governments have choices. The responsiveness of corporate tax burdens to capital formation may, under some governments, be part of a policy mix designed to maintain adequate investment and to address the demands of core constituencies.

A central question for students of political–economic interaction in democratic capitalist nations is to what extent politics shapes the policy choices of authoritative actors within the state. For many, this question amounts to whether or not left-wing governments differ from others in their social and economic policies and impacts (e.g., Castles 1982; Hibbs 1987a, 1987b; Stephens 1979). For some, the question also extends to the role of union movements, neocorporatism, and economic structure in shaping redistributive policies and outcomes (Hicks and Swank 1984a; Muller 1989) and economic policies and performances (Hicks 1988; Lange and Garrett 1985). Still others stress the policy impacts of facets of democratic electoral competition, such as the electoral cycle (Nordhaus 1975; Tufte 1978). Yet an important claim of several observers of democratic capitalism, both neopluralist (Lindblom 1977; Stone 1980) and neo-Marxist (Block 1977; Miliband 1969; Offe 1984), is that the social and economic policies of all governments are fundamentally conditioned by their dependence on the willingness of capitalists to continue to invest. This assertion, dubbed the *structural dependence* (of the state on capital) thesis by Przeworski and Wallerstein (1988), is the central focus of the present inquiry.

The structural dependence thesis is by now familiar.[1] Frequently couched in terms of the "privileged position of business" in extracting investment incentives from the state (Lindblom 1977, chap. 13) or of the "structural power" of capital to constrain the range of state action (Block 1977), the thesis embodies a number of important propositions about the relationship between capitalism, politics, and the state in liberal democracies. After a brief review of the thesis and extant evidence on its accuracy as a theory of state behavior in capitalist democracies, I shall present an analysis designed to evaluate one of the thesis's central claims about the role of capital investment as a determinant of governmental policy choices. I shall focus on the responsiveness of corporate income tax policies to the ebbs and flows of business investment in productive activities in a sample of 16 affluent Western democracies from 1965 to 1984. To provide a comparative benchmark and to explore certain questions that emerge from the principal analyses, I shall also extend the inquiry to cover personal income taxation.

THE STRUCTURAL DEPENDENCE OF THE STATE: THEORY AND EVIDENCE

The bedrock of the structural dependence thesis consists of four sets of propositions about the interaction of politics and economics in democratic capitalist systems.[2] Taken individually, most of these claims seem uncontroversial. First, proponents of the thesis argue that economic performance (output, jobs, price stability, and income) is significantly determined by the willingness of the owners of capital to continue to invest in productive activities. As Lindblom argues: "Because public functions [i.e., investment] in the market system rest in the hands of businessmen, it follows that jobs, prices, production, growth, the standard of living, and the economic security of all rest in their hands. . . . Constitutional rules—especially the laws of private property—specify that, although governments can forbid certain kinds of activity, they cannot command business to perform." (1977, 172–73).

Second, structural dependence theorists argue that the role of investment in capitalist democracies is crucial for state policymakers because of the nature of democratic politics and institutions themselves (Block 1977, 15; Lindblom 1977, chap. 13). In the view of these theorists, as well as most students of political behavior in Western democracies, performance of the economy (income growth, employment levels, and

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inflation) is crucial for the continued popular approval and reelection of incumbent policymakers (Hibbs 1987a, 1987b; Kiewiet 1985; Lewis-Beck 1986). Moreover, maintenance of high and expanding levels of economic activity is necessary for extraction of the revenues that provide the lifeblood of government personnel and programs.[3] Block sums up the argument succinctly: "Those who manage the state apparatus . . . are dependent on the maintenance of some reasonable level of economic activity. This is true for two reasons. First, the capacity of the state to finance itself through taxation or borrowing depends on the state of the economy. . . . Second, public support for a regime will decline sharply if the regime presides over a serious drop in the level of economic activity" (1977, 15).

Third (following from the first two and particularly important to the present inquiry), proponents assert that if governments attempt to pursue adverse economic regulatory policies, dramatically alter the distribution of income through significant changes in transfer and tax policies, and otherwise pursue policies that may necessarily increase taxes directly or indirectly on business, they potentially undercut the confidence of business in a profitable rate of return on investment and so reduce growth. As Offe has put it: "As long as investment decisions are 'free', that is, as long as they obey the rule of maximum expected profitability, the decisive variable . . . is what Kalecki called 'business confidence'. The ultimate political sanction is non-investment or the threat of it" (1984, 244).

Moreover, as some structural dependence theorists suggest, state policymakers may frequently find it necessary to offer incentives to business to maintain or restore adequate capital investment (e.g., Lindblom 1977, chap. 13). According to the logic of the thesis, support-maximizing and revenue-seeking governments will necessarily regularly provide investment incentives in the face of low rates of investment and be loath to pursue policies detrimental to business confidence to any significant degree.[4]

Finally, the structural dependence thesis is unique in its claims about the mechanisms by which capital obtains desired state policies. Distinct from many theories of the state, the structural dependence thesis embodies the notion that policy-oriented collective actions by businessmen and the organizations that represent them are not necessary to secure investment incentives. As some theorists of structural dependence say quite explicitly, individual businessmen need only calculate rationally that under the current mix of extant or proposed policies the rate of return on investment is insufficient (e.g., Lindblom 1977, 170–75). The ensuing downturn in investment is sufficient to elicit policy response in the absence of conscious attempts to do so through collective action.[5]

Evidence

Thus, there is ample evidence to suggest that economic performance directly affects the public's evaluation of government performance and, in turn, the support and votes that the public provides to incumbents. The proposition that the continuation of business investment in productive capacity is crucial for economic performance is also uncontroversial. However, the notion that the state actually behaves in the manner suggested by the thesis has received remarkably little attention.

Some evidence for the thesis as a theory of state behavior is anecdotal. Efforts at significant redistributions of income and wealth in Allende's Chile, Manley's Jamaica, and even Mitterrand's France are said to have led to serious falls in business confidence and, in turn, economic downturn and policy retrenchment. These analyses of "limiting cases" (e.g., Rueschemeyer and Evans 1985) are instructive but do not tell us much about the relationships between the ebbs and flows of capital formation on the one hand and the provision of investment incentives and other policies designed to enhance business confidence on the other. But it is these more routine political–economic interactions that serve as a crucial test of the generalized form of the structural dependence thesis; that is, if (as the thesis maintains) the crucial linkage is between investment-induced growth and disinvestment-induced decline on the one hand, and popular support, votes, and revenues on the other, vote-maximizing and revenue-seeking incumbents with an electoral horizon of three to five years would certainly display some consistent sensitivity to the basic determinants of growth. Downturns in business investment may call forth immediate, direct investment incentives and other policies that eliminate investment disincentives. To paraphrase Keynes, the logic of the thesis suggests that incumbents may be dead in both the short and long term if they do not regularly bolster the conditions that underpin continued investment.[6]

A handful of recent studies provides limited evidence on the responsiveness of government policies to investment in capitalist democracies. In an effort to assess the structural dependence thesis, a small number of studies explicitly examine the responsiveness of corporate income taxation to investment or general economic downturn. In a study of effective corporate tax rates in the postwar United States, Jacobs (1988) reports that there is no relationship between variations in business investment and corporate tax policy. Complementary results are reported by Quinn and Shapiro (1990) in a study of business influences on United States tax policy: variations in capital investment are either unrelated or negatively related to measures of corporate income taxation and to the distribution of tax burdens between owners of capital and wage earners.

On the other hand, Quinn (1988, 1989) has also reported results consistent with the structural dependence thesis. Examining the relationship between investment incentives and economic conditions in France, Great Britain, Japan, the United States, and West Germany, Quinn (1988) finds that governments increase investment incentives during periods of eco-

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nomic downturn and that the sensitivity of corporate tax policies to economic downturn is greater during periods of economic crisis. Similarly, for the United States, Quinn (1989) finds that effective corporate tax rates decline in response to low growth rates and during periods of economic downturn generally. Vogel (1989) reports complementary case study results for the United States that suggest business was able to obtain various concessions from government primarily during the severe downturn in the late 1970s and early 1980s.[7]

Przeworski and Wallerstein (1988) provide the most thorough analysis of the structural dependence thesis to date. However, their central purpose is not to evaluate the thesis as an explanation of the forces that shape state policies but to test the notion that policies that tax the owners of capital to redistribute income must necessarily result in disinvestment. Through a formal analysis, the authors conclude that in the presence of taxes that give preference to investment and penalize consumption out of profits, the thesis is wrong in a static sense: "Any distribution of consumption between wage earners and shareholders is compatible with continual private investment" (p. 24). However, they simultaneously conclude that in a dynamic sense business anticipations of new taxes on their consumption, even with significant investment incentives in place, may well result in disinvestment by capital.[8]

Together, these studies provide very limited information on the accuracy of the structural dependence thesis as a theory of state behavior. As I have mentioned, there is notable inconsistency in their central conclusions about the validity of the thesis. Moreover, with one exception, they are limited to the United States. In addition, Quinn's (1988) comparative study does not directly examine corporate tax responses to variations in capital investment. In sum, the central question of whether or not the state in democratic capitalist political economies systematically responds to low rates of capital investment with investment incentives remains largely unanswered.

Theory, Policy, and Hypotheses

On the basis of the literature and these general theoretical propositions, a number of specific hypotheses about the effect of variations in capital investment on state policy choices can be developed. The logic of the thesis and its interpretation and investigation by scholars suggest that a focus on the responsiveness of corporate tax burdens to business investment is a useful, initial avenue of inquiry for three reasons. First, it effectively indexes both the interplay of corporate tax rates and tax-based investment incentives (e.g., accelerated depreciation allowances, investment tax credits). Indeed, corporate tax rates and tax-based investment incentives have been highlighted by some structural dependence theorists and researchers as constituting two of the principal instruments available to policymakers to bolster low rates of investment. Second, it may assist in answering broader questions posed by the structural dependence thesis. For instance, corporate tax burdens share obvious features of measures of taxes on capital emphasized in studies of redistribution–disinvestment linkages (Przeworski and Wallerstein 1988; Wallerstein 1988). Third, either in terms of rate reductions or investment incentives, it is consistent with both the theory and practice of neo-Keynesian and neoconservative macroeconomic orthodoxy that informed policymakers across my period of investigation.[9]

HYPOTHESIS 1. *Low rates of capital investment will engender reduced taxation of corporate profits.*[10]

Two additional sets of hypotheses stem from theorizing on the determinants of state policy choice in democratic capitalist political economies, from past interpretations and tests of the structural dependence thesis, and from the experiences of democratic capitalist nations in recent years. Each of these departs from a strict understanding of structural dependence that disallows direct or indirect policy impacts of democratic electoral competition and political economic conditions.

Structural Dependence and Party Government. First, extant theory and research on the partisan model of policy choice provide some evidence that the social and economic policies of governments of left-wing and right-wing parties do indeed differ (e.g., Castles 1982; Hibbs 1987a, 1987b; Hicks and Swank 1984a, 1984b; Swank 1988), although both theoretical and empirically based critiques are frequent (Jackman 1986; Pampel and Stryker 1989). Although much of the literature is inconclusive, a number of scholars have hypothesized that left-wing governments may be more likely to favor generally higher levels and rates of taxation on personal income and corporate income in order to promote downward income redistribution to their lower-income constituencies (Cameron 1985; Morrissey and Steinmo 1987; Rose and Karran 1987). Left-wing governments may also be more likely to adopt Keynesian models of the economy that tend to emphasize demand–stimulus responses to low rates of investment and compatibility between high levels of income taxation and redistribution on the one hand and high levels of investment and growth on the other.

HYPOTHESIS 2a. *Corporate tax policies under governments controlled by left-wing parties will be less sensitive to variations in capital investment than such policies under other types of governments.*

HYPOTHESIS 2b. *Corporate tax policies under right-wing governments will be more sensitive to the ebbs and flows of new investment than such policies under other party governments.*

This hypothesis is proposed because of the electoral significance to right-wing parties of business and the owners of capital, because right-wing parties are more likely than others to adopt neoconservative

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Table 1

Corporate Income Taxes and Tax Rates in 16 Western Capitalist Democracies

COUNTRY	CORPORATE INCOME TAXATION AS A % OF CORPORATE PROFITS 1965–69	1980–84	CORPORATE TAX RATES, 1983 (TOTAL STANDARD RATE)[a]
Australia	17.2	15.9	46.0
Austria	7.9	6.5	60.9/36.9
Belgium	6.8	11.2	45.0
Canada	17.4	13.1	47.0/52.0
Denmark	4.2	7.8	40.0
Finland	8.1	7.6	59.0
France	5.7	10.9	50.0
West Germany	8.7	9.4	63.3/46.7
Ireland	8.2	7.2	50.0
Italy	3.7	6.9	40.5
Netherlands	9.7	11.9	43.0
Norway[b]	5.7	29.2	50.8
Sweden	9.6	9.5	57.4
Switzerland	5.7	8.5	14.2/19.3
United Kingdom[b]	12.3	22.9	52.0
United States	18.5	12.5	50.3
16-nation mean	9.3	11.9	47.1
14-nation mean	—	9.9	—

Sources: Taxation and gross domestic product data are from the OECD's *Revenue Statistics of OECD Nations* (selected years) and *National Accounts of OECD Nations* (selected years). Corporate profit rate data are from Sijbren Cnossen, "Alternative Forms of Corporate Taxation," *Australian Tax Forum* 1 (1984): 253–79.

[a] The double numbers indicate effective rates on retained and distributed profits, respectively.

[b] Note that disproportionately high numbers for Norway and the United Kingdom in the second period are due in large part to North Sea oil revenues. For estimates of the effects of North Sea oil revenues on corporate profits taxes see, e.g., OECD's *Economic Survey: Norway* (selected years).

macroeconomic orthodoxies that emphasize the investment disincentives of taxes, and because right-wing governments generally prefer reductions and low levels of taxes of all sorts.

On the other hand, there is evidence in the experience of Scandinavia (in particular) that actual corporate tax burdens under left-wing governments have been relatively low even in the face of high corporate tax rates. As Schwerin (1984), Esping-Anderson (1985), and others have pointed out, social democratic governments in Sweden, Norway, Finland, and Denmark have granted or maintained ample investment allowances (e.g., special tax-free investment funds) in the taxation of retained earnings. For instance, although Sweden maintained one of the highest corporate tax rates in the early 1980s (57.4% in 1983), overall corporate taxation as a percentage of corporate profits was slightly below the mean for 14 affluent democracies. As illustrated in Table 1, the four Scandinavian political economies (with the exception of Norway in the later period) were at or below the group mean for corporate tax burdens in both the late 1960s and early 1980s despite the fact that Finland, Norway, and Sweden's 1983 corporate tax rates were among the highest.

Contrary to partisan theory, the Scandinavian experience raises the prospect that left-wing governments in general may be sensitive to fluctuations in investment in new productive capacity. One plausible reason for this may be gleaned, in part, from partisan theory itself. Because left-wing governments are hypothesized to pursue redistributive policy goals through significant expansions of social welfare outlays (Hicks and Swank 1984b, 1990; Stephens 1979), high levels of progressive personal income taxation (Cameron 1985), labor market policies, and so on, these governments may be unusually attentive to the investment disincentives of these policies hypothesized by some neoconservative macroeconomic theories.[11] In addition, as I have noted, even the notably strong Keynesian orientation of most of these left-wing governments would allow for the use of investment incentives in response to low rates of capital investment.[12] Finally, within the context of potential growth–equity trade-offs, left-wing governments may forego high tax burdens on reinvested business profits to promote the future welfare of labor. From the perspective of left-wing parties within democratic capitalism, future jobs and income of workers might be promoted by ample current investment incentives and concomitant low corporate tax burdens (see also Przeworski and Wallerstein 1982, 1988).

HYPOTHESIS 2c. *Corporate income taxation under left-wing governments will be more responsive to low capital investment than under other governments.*

Structural Dependence and Economic Crisis. In addition, the logic of the thesis, past studies, and recent experience provide a third set of hypotheses. These

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hypotheses revolve around the proposition that government tax policy responses to low rates of capital investment will be more pronounced during periods of economic stagnation. Specifically, the responsiveness of corporate tax policies should be more pronounced during the post-1973 era of stagflation that has plagued democratic capitalist systems. This hypothesis is justified for two reasons. First, sustained periods of slow growth, high unemployment, and price instability should intensify the sensitivity of support-maximizing and revenue-seeking incumbent policymakers to the underpinnings of growth and productivity, particularly capital investment. Second, the post-1973 crisis was accompanied by the ascendance of neoconservative macroeconomic orthodoxies that shifted policymaker attention toward the supply-side determinants of growth, particularly the after-tax rate of return on investment (Bosworth 1984; Przeworski and Wallerstein 1982).

With regard to specific political economic eras, one can suggest that the 1973 OPEC oil price shock serves to demarcate the beginning of the period of ascendance of neoconservative economic orthodoxy, stagnation, and high inflation and interest rates (Bruno and Sachs 1985; Swank 1988). One might also suggest that the second OPEC shock of 1979–80 served to underscore—even intensify—the effects of the first crisis and contribute significantly to the rise of neoconservative ideas and governments (Tsai 1989). On the other hand, the widespread restrictive tilt of macroeconomic policy combined with (automatic) prodeficit pressures of continued stagnation among the affluent capitalist democracies may have raised barriers to systematic cuts in corporate tax burdens in the post-1979 era.

HYPOTHESIS 3a. *Corporate tax policy was more responsive to investment in the post-1973 period than before.*

HYPOTHESIS 3b. *Corporate tax policy was more responsive to trends in capital investment during OPEC 1 than in other years.*

Alternatively, HYPOTHESIS 3c. *Policy was more responsive during OPEC 2 than in other years.*

THE FRAMEWORK FOR ANALYSIS

Quantitative analyses of my central questions are conducted within the framework of a pooled time series design. This choice was made largely because cross-nationally and temporally comparable quantitative indicators for all of the hypothesized explanatory factors are only available for a relatively short time interval (the early 1960s to the mid-1980s) and for a relatively small group of capitalist democracies. Thus, from a conventional perspective on cross-national empirical analyses, the choices were between individual country analyses on approximately 20 data points or cross-sectional analysis of 16–18 data points. Both of these options were rejected in favor of an analysis of pooled cross-sections of time series. This procedure maximizes the available sample size and allows more accurate specifications of the processes determining state policy choices. The actual sample was limited to time series of 20 annual observations from 16 nations.[13]

Model Specification

The generalized model for testing the impacts of movements in business investment on corporate tax burdens may be represented as follows:

$$\text{POLICY}_{it} = B_{1i} + \sum_{k=2}^{k} B_k X_{kit} + e_{it},$$

where i and t are observations on units (nations) and time points (years), respectively, B_{1i} is an equation intercept or intercepts, B_K is a vector of coefficients for the matrix of observations on k exogenous variables (X_K), and e is the error term. Within the X matrix, observations on a variable indexing the movements of business investment in productive capacity and four sets of variables that represent alternative views of the determination of tax policy choices and essential controls are included.

The first set of variables controls for two important potential determinants of corporate tax burdens: the volume of profits itself and the automatic tax effects of investment. With regard to profits, some automatic responsiveness of corporate taxes to variations in the volume of profits is built into tax policy.[14] In addition, policymakers may have made conscious tax policy choices in response to low profits, given the role of profits in generating investment in neo-Keynesian and neoconservative models of investment. Generally, a finding of policy responsiveness to profits might indicate further, indirect support for structural dependence propositions about government sensitivity to investment. Concerning automatic effects of investment on corporate tax burdens, it is important to keep in mind that increases in investment will automatically produce declines in tax burdens (and decreases will produce rises), net of other factors, because of the near-universal use of accelerated depreciation and investment tax credits among our focal countries during the time frame of the present study. Thus, a separate control for this automatic relationship will be used.[15]

With respect to sets of alternative explanatory factors, empirical models incorporate controls for the hypothesized effects of two central aspects of democratic politics. Specifically, following the preceding discussion of left-wing and right-wing party policy preferences, the model allows for the possibility of direct effects of alternations in party government on corporate tax burdens. The model also allows for the prospect that centrist parties (i.e., secular and Christian Democratic centrist parties) are more likely to prefer higher levels of taxation generally than parties of the Right (Cameron 1985). To make actual statistical estimation tractable, the model is initially speci-

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fied with measures of left-wing and centrist party control of government, although extended analyses incorporate right-wing party variables. In addition, following some studies on tax policy formation (e.g., Rose and Karran 1987), a control for the impact of the electoral cycle on corporate tax policies will be employed. As proponents of this familiar thesis assert, vote-maximizing, incumbent politicians seeking reelection may seek to decrease unemployment and increase personal income via transfer, tax, and macroeconomic policies as elections grow near (Nordhaus 1975; Tufte 1978).

The third basic set of factors involves controls for variations in macroeconomic performance—economic growth and inflation rates. Generally, these factors account for the roles of the operation of automatic macroeconomic stabilizers embedded in tax policy, as well as the roles of discretionary responses of policymakers to variations in economic performance. Given that investment in new productive capacity is highly correlated with the business cycle (i.e., gross domestic product growth rates), they may also serve to isolate investment–tax policy relationships that are not part of larger countercyclical processes.

Finally, the model incorporates the possibility that international economic forces have significant, direct consequences for tax policies. First, the model accounts for economic openness of the nation at a particular point in time. As Cameron (1978), Katzenstein (1985), and others have argued, high levels of exposure to international competition in goods and services and of flows in labor and capital create pressures for various forms of state intervention to facilitate competitiveness and to compensate groups for adjustments to international competition. In the long term, international economic openness may also contribute to the strength of political organizations (i.e., left-wing parties and union movements) that favor a large state. Together, these forces may create pressures for generally higher levels of taxation, social spending, and state intervention in the economy, as Cameron's (1978) well-known study indicates. On the other hand, openness to the international economy may promote low levels of taxation on business as the state attempts to cultivate international competitiveness, attract new capital investment, and prevent capital flight.

The model also accounts for the possibility of direct effects of energy price shocks associated with actions of OPEC in 1973–74 and 1979–80. As a number of analyses have recently suggested, the OPEC price shocks had important impacts on spending and taxing policy (e.g., Bruno and Sachs 1985; Tsai 1989). These impacts have been direct (through unemployment and price-level effects) and indirect (through shifts in political and intellectual climates and through structural economic changes). Generally, the budgetary impact of these consequences has been to increase spending and deplete revenues through discretionary and automatic policy responses (Chouraqui and Montador 1984), although the post-1973 experience of the OECD nations also suggests that the inflationary aspect of the shocks frequently led to pressures for discretionary fiscal contraction in the form of spending cuts and tax increases, particularly in the post-1979 period (Tsai 1989).

Measurement

The principal indicators for corporate income taxation and the explanatory factors, as well as sources of all data upon which these measures are based, are given in the Appendix. Construction of the empirical indicator for corporate income taxation is relatively straightforward. To adjust for population change, affluence, and profitability and to obviate the problems of heteroscedastic errors typically generated by models where levels of endogenous variables differ notably, total corporate income taxes were expressed as percentages of net operating income (TAXES ON PROFITS).

The operationalization of business investment (real outlays for machinery and equipment) does require slightly more discussion. Ideally, one would employ an indicator of private sector investment in new plant and equipment. However, national account statistics (the principal source of such information) do not provide cross-nationally comparable measures of private sector plant and equipment outlays for the time frame of the present study. Specifically, OECD and UN national account statistics only provide such data for seven nations since the early 1970s. Some alternatives consist of aggregate gross fixed capital formation (GFCF), GFCF net of residential construction, and GFCF net of general government capital outlays. Unfortunately, all these options leave substantial components of direct government and/or aggregate public sector (general government and public enterprise) investment in the resulting indicator.

One category of capital outlays available for most OECD nations since the early 1960s is relatively largely composed of private sector outlays for new productive capacity, namely, investment in machinery and equipment. For those nations for which purely private sector plant and equipment data are available, 86.4% of total investment in machinery and equipment was made by private sector capital.[16] Moreover, total machinery and equipment outlays covary almost perfectly across time with total private sector nonresidential structure, machinery, and equipment outlays. As a result of these considerations, I shall measure private business investment by total machinery and equipment outlays. Consistent with the theory's central focus on changes in productive investment (e.g., disinvestment), as opposed to particular levels of investment, the actual indicator is expressed as the percentage rate of change in real machinery and equipment outlays (%CH INVESTMENT).

To control for the automatic, negative relationship between investment and corporate profits taxation, the level of investment in the preceding year is used. Specifically, this factor is operationalized as total

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machinery and equipment outlays expressed as a percentage of gross domestic product (LEV INVESTMENT).

The operationalizations of profits and macroeconomic performance variables are relatively straightforward. In the absence of more detailed, cross-nationally comparable indicators, profits are defined here as net operating income of business. To control for changes in population, economic size, and affluence over time, net operating income is expressed as a percentage of national income (PROFIT/INCOME). Operationalization of both economic growth and inflation follow convention: growth is measured as annual percentage changes in real gross domestic product (GROWTH), while inflation is measured as annual percentage changes in the consumer price index (INFLATION).

Operationalizations of openness and the OPEC oil price shocks are also relatively straightforward. Although one might advance more sophisticated indicators of exposure and vulnerability to the upheavals of international labor, capital, and goods markets, I use the conventional indicator utilized by Cameron (1978) and others to measure domestic political economic exposure to international market forces, namely, import and export flows as percentages of GDP (OPENNESS). The actual indicators for the OPEC oil shocks ultimately consist of dichotomous variables (OPEC1 $\text{DUMMY}_{1974\text{–}79}$ and OPEC2 $\text{DUMMY}_{1980\text{–}84}$) designed to capture the effect of the price shocks and their political and economic consequences on policies in the period after the particular economic shock. These indicators were selected because of high collinearity between more specific measures of oil import costs and macroeconomic performance variables.

With respect to the political factors, a dichotomous variable coded 1.00 for an election year and .00 otherwise (ELECTION) was used to measure the potential policy effects of electoral cycle forces. Consistent with past work on the impacts of partisan control of government (Cameron 1978; Hicks and Swank 1990; Swank 1988), political party control of government was operationalized by computing the annual averages of the proportion of cabinet portfolios held by left-wing (LEFT CABINET), centrist (CENTER CABINET), and right-wing (RIGHT CABINET) parties as defined in Hicks and Swank (1990).

Concerning specification of time lags and leads appropriate to evaluating the focal hypotheses, all variables were lagged one period (or led in the case of election years) except OPEC1 and OPEC2. This was done to obviate simultaneity bias potentially present in some of the coefficients and to allow for both automatic and discretionary processes to begin to exert themselves on policy.[17] With regard to OPEC1 and OPEC2, since price effects and their short-term consequences may appear rather quickly, since I am attempting to measure period effects, since the year specifications (1974–79 and 1980–84) already involve some lag from the oil shocks, and since no compelling reason exists for a specific alternative, I made a contemporaneous specification.

In light of these measurement stipulations and earlier theoretical discussion, the following baseline empirical model may be offered:

$$\text{TAXES ON PROFITS}_{it} = B_{1i} + \sum_{k=2}^{k} B_k X_{kit} + e_{it},$$

where X consists of $\%\text{CH INVESTMENT}_{t-2 \text{ to } t-1}$, $\text{PROFITS/INCOME}_{t-1}$, $\text{LEV INVESTMENT}_{t-1}$, $\text{LEFT CABINET}_{t-1}$, $\text{CENTER CABINET}_{t-1}$, ELECTION_{t+1}, $\text{GROWTH}_{t-2 \text{ to } t-1}$, $\text{INFLATION}_{t-2 \text{ to } t-1}$, OPENNESS_{t-1}, OPEC1 $\text{DUMMY}_{1974\text{–}79}$, and OPEC2 $\text{DUMMY}_{1980\text{–}84}$.[18] This model, as well as variants of it incorporating nonlinear or interactive effects, will be used to evaluate hypotheses 1–3c.

Estimation

The pooling of time series (t = 20) from a number of cross-sectional units (n = 16) presents four formidable estimation difficulties unless the data are unusually well behaved. First, troublesome autocorrelation of errors may be present within cross sections. Second, errors may be heteroscedastic across individual units, groups of cross sections, or blocks of time. Third, errors may be correlated across cross sections at the same point in time. Fourth, the ordinary least squares (OLS) intercepts for the individual cross sections may be notably different from one another. The sum of these problems, if present, constitutes serious violations of OLS assumptions about error structure and will generally produce biased OLS coefficient and standard error estimates (Johnston 1984, chap. 10; Kmenta 1971, chap. 12; Stimson 1985). In the current analyses, the presence of all these problems is possible: the temporally dominant nature of the model (T > N) suggests concern for autoregressive errors, while the nature of the units and endogenous variables indicates potential heteroscedasticity. Moreover, cross-sectionally correlated errors are likely when pooling time series of data from geographic entities (Kmenta 1971, 512). Nation-specific intercepts for tax equations are also likely given the wide variability of political and economic structures that cannot be captured in our models.

To obviate the risks of these potential problems, my models and extensions were estimated using Kmenta's generalized least squares procedure (1971, equations 12.22–40). This method corrects for cross-sectional heteroscedasticity, contemporaneous cross-sectional correlation of errors, and first-order autoregressive errors.[19] To account for the remaining possibility of intercept differences across units, dummy variables (N − 1) were added to the equations. The presence of these variables is likely to bias downward the coefficients of variables whose effects are partially cross-sectional (Judge et al. 1985, chap. 13). The potential bias in all estimates when significant unit effects are present and ignored suggests their inclusion, particularly in the absence of more theoretically informed cross-sectional controls (Stimson 1985). Finally, to maximize the probability of

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correctly adjusting estimates for the autoregressive error component, Kmenta's Full Cross-Sectionally Correlated and Autoregressive Model was estimated with individual-nation estimates of first-order autocorrelation (Kmenta 1971, equation 2, 12.25–29). Finally, in order to estimate the conditional effects of business investment on corporate tax policies, interaction analysis was employed. That is, for all focal hypotheses that suggest that effects of business investment on corporate tax burdens vary by the level of another variable, interactions and conditional coefficients and associated significance tests were computed.[20]

FINDINGS

The results of estimation of the baseline corporate income tax model are presented in the first set of columns of Table 2. For all models, the upper portion of the table reports generalized least squares estimates of the parameters for the focal causal factors while the middle portion reports nation-specific intercepts. The reader should recall that these parameters index unit effects relative to the left-out unit (the United States), net of the effects of other exogenous variables. As such, they have little theoretical meaning and are presented for the information of the interested reader. The lower portion of the table reports goodness of fit and residual statistics.

As the first column of Table 2 reveals, support for the principal proposition derived from the structural dependence thesis is generated from the estimation of our baseline model: as the rate of new investment declines corporate tax burdens decline (and as the rate increases, so do the burdens). Moreover, this significant parameter estimate ($b = .0182$, $t = 3.96$) relating the rate of investment in new productive capacity and corporate tax burdens emerges in the presence of controls for alternative political and economic determinants of corporate income taxation and for the automatic effects of investment on corporate tax burdens: the relationship between past levels of investment and corporate tax burdens is negative and significant, as expected. Profits and corporate income taxation are also significantly associated, as anticipated: low profitability is associated with low corporate tax burdens, and high profitability with high burdens. While it is impossible to segregate automatic and discretionary policy effects of variations in profits, the arguably limited automatic linkages of profits and taxation and the role of profits as a determinant of investment in economic theory suggest that an investment-motivated discretionary response to profitability is a possibility. As such, a discretionary profits effect would underscore structural dependence propositions about the sensitivity of policymakers to investment and its determinants.

With respect to the direct effects of political forces, left-wing and centrist party control are both negatively and significantly related to corporate income taxes. While this finding supports the view gleaned from the Scandinavian experience about left-wing parties and corporate tax policy, it also extends it to Centrist parties: net of other forces, centrist parties such as the centrist Christian Democratic parties of the Benelux nations also maintain lower corporate tax burdens. Although correctly signed, the parameter estimate for the election year variable is completely insignificant.

Table 2 also reports findings for effects of macroeconomic performance and international economic factors. In the case of growth and inflation, findings are consistent with theory on countercyclical movements of taxes: corporate tax burdens are positively and significantly related to rates of change in real gross domestic product and in price levels. With respect to international economic factors, findings for openness indicate that exposure to international markets, net of other forces, results in pressures for low corporate taxes. On the other hand, the direct effects of the OPEC oil shocks are positive: net of other determinants of corporate tax policy, corporate tax burdens were higher in both OPEC 1 ($b = 1.8990$) and OPEC 2 ($b = 3.2087$). These findings may be interpreted as reflecting the overall upward movement in taxes in the last several decades, as well as pressures on taxes generated by restrictive fiscal policy and stagnation-induced budget deficits, particularly in the latter period.

Overall, results from the baseline model provide clear support for the notion that low rates of investment in new productive capacity systematically give rise to reductions in corporate tax burdens. However, two qualifications of this initial conclusion must be made. First, the magnitude of policy response to investment is arguably not large. For instance, a decline from 10% to 0% in the rate of new investment is associated with a .2 reduction of corporate income taxes as a percentage of net operating income. Given that the average of corporate taxes as a percentage of net operating income is roughly 10% across our focal period (see Table 1), this relative change is not great. Secondly, results reported in Table 2 suggest that a number of other political and economic forces exert substantial influence in corporate tax policymaking. For instance, the advent of a left-wing government (LEFT CABINET from .00 to 100) would have a larger impact on corporate taxes than a 10% fall in the rate of new investment. In sum, particularly strong inferences from structural dependence theory about, for example, the overriding role of investment and the trivial import of democratic elections could not be made on the basis of our otherwise clear evidence for the thesis.[21]

Results for tests of the hypotheses proposing that policy responsiveness to investment will be conditioned by the partisan composition of government are presented in the second and third pairs of columns of Table 2. These columns provide information on the interaction itself and the conditional coefficients for the policy impact of investment at party government levels of 40% and 100% of cabinet portfolios. As the second pair reveals, the interaction between invest-

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Table 2

Capital Investment, Politics, and Corporate Income Taxation, 1965–84: Direct and Partisan-mediated Investment Impacts on Corporate Profits Taxes in 16 Nations

VARIABLE	BASELINE MODEL UNSTAND. GLS EST.	BASELINE MODEL SE	INVESTMENT AND TAXES UNDER RIGHT-WING GOVERNMENTS UNSTAND. GLS EST.	INVESTMENT AND TAXES UNDER RIGHT-WING GOVERNMENTS SE	INVESTMENT AND TAXES UNDER LEFT-WING GOVERNMENTS UNSTAND. GLS EST.	INVESTMENT AND TAXES UNDER LEFT-WING GOVERNMENTS SE
%CH INVESTMENT$_{t-2 \text{ to } t-1}$	.0182*	.0046	.0138*	.0050	.0063	.0054
PROFIT/INCOME$_{t-1}$	.0088*	.0023	.0847*	.0023	.0764*	.0232
LEV INVESTMENT$_{t-1}$	−.1482*	.0527	−.1553*	.0484	−.2349*	.0526
RIGHT CABINET$_{t-1}$	—	—	.0044*	.0020	—	—
RIGHT * %CH INVEST	—	—	.0002*	.0001	—	—
%CH INVEST at						
RIGHT$_{40}$	—	—	.0216*	.0047	—	—
RIGHT$_{100}$	—	—	.0334*	.0091	—	—
LEFT CABINET$_{t-1}$	−.0060*	.0019	—	—	−.0064*	.0019
LEFT * %CH INVEST	—	—	—	—	.0004*	.0001
%CH INVEST at						
LEFT$_{40}$	—	—	—	—	.0245*	.0044
LEFT$_{100}$	—	—	—	—	.0518*	.0068
CENTER CABINET$_{t-1}$	−.0066*	.0026	—	—	−.0054*	.0025
ELECTION$_{t+1}$	−.0095	.0463	−.0292	.0430	−.0502	.0478
GROWTH$_{t-2 \text{ to } t-1}$	.0440*	.0137	.0494*	.0127	.0483*	.0138
INFLATION$_{t-2 \text{ to } 1-1}$	.0735*	.0176	.0814*	.0175	.0880*	.0172
OPENNESS$_{T-1}$	−.0456*	.0086	−.0444*	.0084	−.0452*	.0082
OPEC1 DUMMY$_{1974-79}$	1.8990*	.1850	1.7957*	.1812	1.8207*	.1814
OPEC2 DUMMY$_{1980-84}$	3.2087*	.2362	3.1691*	.2280	3.0647*	.2296
AUSTRALIA	1.0525	1.9863	.0944	1.9303	1.1980	11.9866
AUSTRIA	−5.4872*	1.8784	−5.6604*	1.7767	−5.3859*	1.8844
BELGIUM	−1.2807	2.1718	−1.6246	2.0863	−1.4956	2.1422
CANADA	.6152	2.1541	.6984	2.0653	.2090	2.1394
DENMARK	−7.2370*	1.9827	−7.4554*	1.8929	−7.4697*	1.9856
FINLAND	−5.6503*	1.9397	−5.8613*	1.8577	−5.7197*	1.9541
FRANCE	−5.8573*	2.0791	−5.9983*	2.0011	−5.7630*	2.0562
W. GERMANY	−4.6478*	1.8154	−4.8042*	1.7370	−4.6605*	1.8234
IRELAND	−4.3929*	1.9896	−4.5623*	1.9457	−4.2732*	1.9710
ITALY	−11.0490*	2.0899	−11.2740*	2.0059	−11.0550*	2.0894
NETHERLANDS	−.0779	2.0224	−.3508	1.9228	−.1226	2.0260
NORWAY	4.2550	3.8603	3.3989	3.7459	4.8384	3.5753
SWEDEN	−2.8630	1.8674	−3.1361*	1.7862	−3.1816*	1.8806
SWITZERLAND	−4.9155*	2.0294	−5.1480*	1.9326	−4.9451*	2.0361
U. K.	3.5529	2.5355	3.3573	2.5027	3.2473	2.5194
Constant	12.9230*	2.0175	12.5960*	1.9169	13.9830*	2.0349
Degrees of freedom	293		293		292	
Buse R^2	.8108		.8431		.8510	
Durbin–Watson	1.9633		1.9702		1.9498	
Rho	.0134		.0114		.0190	

Note: These pooled time-series estimates are obtained by estimating Kmenta's Full Cross-sectionally Correlated and Autoregressive (AR1) Model with unit effects added (Kmenta 1971, equations 12.33–40).
*$p \leq .05$.

ment and right-wing party government control is significant: as right-wing party control increases, the effect of investment on corporate taxation increases. This finding is placed in concrete relief by the two conditional coefficients. At the level of 40% right-wing party cabinet portfolios, the coefficient relating investment to taxation is .0216, while at the 100% level, it is .0334.

The third pair of columns of Table 2 reports the results for the left-wing party–investment interaction. As suggested by my alternative hypothesis (2c) for the mediating effect of left-wing parties, left-wing parties appear to be notably sensitive to trends in investment. The interaction is positive and significant, indicating that as left-wing party government control increases, the policy effects of investment are

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Table 3

Capital Investment, Economic Crisis, and Corporate Income Taxation, 1965–84: Investment Impacts on Corporate Profits Taxes during Economic Crisis in 16 Nations

	INVESTMENT AND TAXES					
	DURING OPEC		DURING OPEC1		DURING OPEC2	
VARIABLES	GLS EST.	SE	GLS EST.	SE	GLS EST.	SE
%CH INVESTMENT$_{t-2 \text{ to } t-1}$	.0201*	.0068	.0131*	.0050	.0185*	.0054
PROFIT/INCOME$_{t-1}$	.0767*	.0243	.0847*	.0242	.0749*	.0246
LEV INVESTMENT$_{t-1}$	−.2215*	.0563	−.1611*	.0529	−.1525*	.0567
LEFT CABINET$_{t-1}$	−.0072*	.0017	−.0059*	.0019	−.0064*	.0019
CENTER CABINET$_{t-1}$	−.0088*	.0025	−.0071*	.0026	−.0083*	.0028
ELECTION$_{t+1}$	−.0143	.0517	−.0179	.0451	−.0474	.0495
GROWTH$_{t-2 \text{ to } t-1}$	.0213	.0150	.0419*	.0139	.0496*	.0144
INFLATION$_{t-2 \text{ to } t-1}$	.0574*	.0188	.0756*	.0180	.0751*	.0183
OPENNESS$_{t-1}$	−.0098	.0078	−.0451*	.0085	−.0477*	.0090
OPEC DUMMY$_{1974-84}$	1.8464*	.2107	—	—	—	—
OPEC * %CH INVEST	.0048	.0078	—	—	—	—
opec1 DUMMY$_{1974-79}$	—	—	1.8155*	.1929	1.8893*	.1938
OPEC1 * %CH INVEST	—	—	.0185*	.0071	—	—
%CH INVEST in OPEC1	—	—	.0316*	.0066	—	—
OPEC2 DUMMY$_{1980-84}$	—	—	3.1256*	.2411	3.1694*	.2470
OPEC2 * %CH INVEST	—	—	—	—	−.0102	.0089
AUSTRALIA	.3908	1.8707	1.1234	2.0178	1.0979	2.0043
AUSTRIA	−7.1037*	1.7370	−5.3862*	1.9001	−5.2741*	1.8923
BELGIUM	−4.5414*	1.9980	−1.2077	2.1876	−.9050	2.2094
CANADA	−.6259	1.9493	.3569	2.0794	.8194	2.1223
DENMARK	−8.9999*	1.8730	−7.1973*	2.0064	−7.0542*	2.0054
FINLAND	−6.8459*	1.8009	−5.5325*	1.9566	−5.4071*	1.9549
FRANCE	−6.5831*	1.9900	−5.7273*	2.0991	−5.7559*	2.1117
W. GERMANY	−5.8207*	1.6622	−4.5757*	1.8363	−4.4873*	1.8331
IRELAND	−7.1176*	1.8507	−4.3539*	2.0075	−4.1954*	2.0178
ITALY	−11.5380*	2.0325	−10.8800*	2.1319	−10.5000*	2.0914
NETHERLANDS	−2.9405	1.8625	−.0366	2.0401	.2229	2.0581
NORWAY	1.2137	3.7500	4.3866	3.8502	4.3851	3.9570
SWEDEN	−4.2709*	1.7375	−2.8543	1.8932	−2.7561	1.8834
SWITZERLAND	−6.6321*	1.8822	−4.8203*	2.0515	−4.6815*	2.0378
U. K.	1.9714	2.4574	3.5954	2.5630	3.5107	2.5466
Constant	14.0090*	1.8515	13.0870*	2.0338	13.270*	2.0398
Degrees of freedom	293		292		292	
Buse R^2	.7760		.8104		.8097	
Durbin–Watson	1.9730		1.9620		1.9401	
Rho	.0128		.0139		.0238	

Note: These pooled time-series estimates are obtained by estimating Kmenta's Full Cross-sectionally Correlated and Autoregressive (AR1) Model with units effects added (Kmenta 1971, equations 12.33–40).
*$p \leq .05$

magnified. The conditional coefficients actually indicate that the effect of investment in new productive capacity on corporate tax burdens is higher during a left-wing government that controls all cabinet portfolios (b = .0518) than for a comparable right-wing government (b = .0334).[22] As such, this finding, combined with evidence on the direct effects of left-wing parties from the baseline model, adds weight to the propositions that left-wing governments (for the reasons I have discussed) are likely to maintain low corporate tax burdens and be particularly sensitive to trends in new capital investment when formulating corporate tax policy.

Table 3 reports the results of tests of the hypotheses predicting that corporate tax policy responsiveness to rates of new investment will be more pronounced during periods of economic crisis. As reported in the first pair of columns, the interaction between the before-or-after OPEC 1 variable (OPEC DUMMY$_{1974-84}$) and capital investment, while correctly signed, is not significant. This finding indicates no overall difference in the policy impacts of capital investment across the two periods. However, interactions between OPEC subperiods and the capital investment variable indicate that we should not reject the economic crisis hypothesis too quickly. As the second column pair of Table 3 reveals, there is a significant interaction between OPEC1 DUMMY$_{1974-79}$

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and capital investment; that is, policy responsiveness is greater in the years 1974–79 than in other periods. However, the interaction between OPEC2 DUMMY$_{1980-84}$ and capital investment is insignificant, indicating that policy responsiveness to investment in the years 1979–84 did not differ significantly from other periods.

Together, these findings suggest that the effects of political economic changes occurring in the wake of the first OPEC oil shock intensified policy sensitivity to capital investment but that this heightened responsiveness was not sustained after 1979. Generally, this pattern of findings bolsters support for the proposition that the restrictive turn in macroeconomic policy among most capitalist democracies after the late 1970s, together with the burgeoning budget deficits in many nations, set limits to significant reductions in tax burdens during these years, net of other forces.[23]

Capital Investment, Politics, and Personal Income Taxation

A good deal of the debate that has emerged since the late 1960s over the disincentive effects of taxation on growth has hinged on discussions of the impact of high levels and rates of personal income taxation on saving incentives and investment (e.g., Bosworth 1984; Feldstein 1987). In addition, even in the era of pronounced neo-Keynesian orientation of policymakers, cuts in personal income taxes to bolster aggregate demand in response to lagging rates of investment would be fully sanctioned. Thus, to expand the empirical investigation and to provide a benchmark for judging the generalizability of the findings in my analysis of corporate taxes, I shall explore hypotheses directly analogous to those generated for corporate income taxes in the case of personal income taxes.[24]

The focus on personal income taxes will, moreover, enable us more fully to explore one of the most novel findings in the preceding analyses of structural dependence propositions—left-wing parties are particularly sensitive to trends in investment in new productive capacity. As I have noted, one explanation for this relationship involves, in part, the notion that left-wing parties maintain high levels of progressive personal income taxes (and social welfare effort) and thus are particularly sensitive to the perceived need to bolster investment through corporate tax-based incentives. This explanation might be strengthened by examining the impact of left-wing governments on personal income taxes within the same framework. Although evidence of partisan effects on welfare effort and economic policies and outcomes is substantial, theoretical and empirical controversies remain. In addition, evidence of systematic partisan effects on income taxation is mixed; few studies have focused systematically on partisan effects on personal income taxes across a large number of countries.

In order to explore these questions, models directly analogous to those already estimated were formulated for personal income taxes. Other than a different dependent variable (TAXES ON INCOME-personal income taxes as a percentage of national income), there are only two minor changes in the baseline model. First, rather than a control for profits, the model includes a control for per capita real national income (REAL INCOME/POP$_{t-1}$), a major determinant of cyclical variation in personal income taxes, and a source of support for high tax burdens (Cameron 1978). Second, the control for the level of investment is dropped. Otherwise, to facilitate comparison to the corporate tax models, all specifications, measurements, and estimation procedures are identical to those for corporate tax equations.

The results for the estimation of the baseline model are presented in the first pair of columns in Table 4. As the table reveals, there is no direct effect of the rate of investment on personal income taxes across our entire period. While correctly signed (b = .0002), the coefficient is tiny in magnitude and completely insignificant. The results reported from the estimation of the baseline model are, however, more supportive of our expectations about the effects of left-wing parties: left-wing party government control is positively and significantly correlated with personal income taxes, net of other forces shaping personal income tax policy. Concretely, the advent of a left-wing government (.00–100% of cabinet portfolios) would increase personal income taxes by .5% of national income. Overall, these findings add weight to the proposition that left-wing parties maintain low corporate taxes and display relatively substantial sensitivity to low rates of investment in the form of adaptive corporate tax policies as part of a coherent policy mix. This mix of policies is ostensibly oriented to producing more income equality while simultaneously generating adequate investment in new productive capacity.[25]

With regard to the control variables in this baseline estimation, policy effects of other exogenous variables are, for the most part, consistent with theory. Real per capita income (REAL INCOME/POP$_{t-1}$), centrist party government control (CENTER CABINET$_{t-1}$), economic openness (OPEN$_{t-1}$), and the OPEC oil shocks (OPEC1 and OPEC2 dummies) are all positively and significantly associated with personal income taxation. Inflation is also positively and significantly associated with personal income taxes, indicating that inflation resulted in bracket-creep increases in these taxes and perhaps led to more restrictive fiscal policies. The only unanticipated findings pertain to election years (ELECTION$_{t+1}$) and economic growth rates (GROWTH$_{t-2 \text{ to } t-1}$): the election years variable is completely insignificant, while the growth variable is, somewhat anomalously, negative and significant. This latter finding (that low rates of growth are associated with high personal income taxes and high rates with low taxes) is probably an artifact of the control for real income. The two variables are highly correlated temporally within countries, and variations in per capita real income probably tap the countercyclical and discretionary growth responses of personal income taxes better than growth rates in gross domestic product.

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Table 4

Capital Investment, Economic Crisis, and Personal Income Taxation in 16 Nations, 1965–84: Investment Impacts on Personal Income Taxes (Baseline and Economic Crisis Models)

			INVESTMENT AND TAXES			
	BASELINE MODEL		DURING OPEC1		DURING OPEC2	
VARIABLE	GLS EST.	SE	GLS EST.	SE	GLS EST.	SE
%CH INVESTMENT$_{t-2 \text{ to } t-1}$	.0002	.0019	−.0034	.0023	.0003	.0021
REAL INCOME/POP$_{t-1}$	.1544*	.0197	.1546*	.0190	.1550*	.0195
LEFT CABINET$_{t-1}$	.0050*	.0008	.0052*	.0008	.0052*	.0008
CENTER CABINET$_{t-1}$	.0036*	.0007	.0035*	.0007	.0036*	.0007
ELECTION$_{t+1}$	.0185	.0226	.0282	.0214	.0167	.0231
GROWTH$_{t-2 \text{ to } t-1}$	−.0264*	.0058	−.0285*	.0055	−.0268*	.0058
INFLATION$_{t-2 \text{ to } t-1}$	.0039*	.0064	.0341*	.0063	.0340*	.0064
OPENNESS$_{t-1}$	.0149*	.0054	.0487*	.0051	.0493*	.0054
OPEC1 DUMMY$_{1974-79}$	1.0550*	.1414	1.0077*	.1401	1.0589*	.1410
OPEC1 * %CH INVEST	—	—	.0087*	.0033	—	—
%CH INVEST in OPEC1	—	—	.0053*	.0028	—	—
OPEC2 DUMMY$_{1980-84}$	1.1358*	.1991	1.1526*	.1977	1.1613*	.1993
OPEC2 * %CH INVEST	—	—	—	—	.0003	.0021
AUSTRALIA	1.4561*	.7481	1.4848*	.7130	1.4543*	.7312
AUSTRIA	−3.2164*	.4231	−3.2580*	.4062	−3.2330*	.4234
BELGIUM	−1.8354	1.0797	−1.7014	1.0786	−1.8241*	1.0749
CANADA	−.9953*	.4062	−.9216*	.3690	−.9798*	.3970
DENMARK	9.1723*	2.3532	9.3357*	2.3505	9.2095*	2.3459
FINLAND	4.5962*	.8115	4.6666*	.8038	4.6159*	.8103
FRANCE	−6.2023*	.4959	−6.2568*	.4853	−6.2216*	.4978
W. GERMANY	−.7786*	.3684	−.8315*	.3725	−.7893*	.3712
IRELAND	−4.8088*	.8383	−4.7196*	.8222	−4.8027*	.8346
ITALY	−5.8701*	.7432	−5.8502*	.7275	−5.8626*	.7430
NETHERLANDS	−2.9299*	.9783	−2.9753*	.9406	−2.9441*	.9748
NORWAY	1.2753	1.5288	1.0068	1.5168	1.1862	1.5435
SWEDEN	8.7008*	.3288	8.6857*	.3169	8.6986*	.3282
SWITZERLAND	−3.1609*	.4595	−3.1502*	.4504	−3.1609*	.4589
U. K.	−.4143	.3266	−.3393	.5455	−.3986*	.5638
Constant	7.7314*	.3266	7.7845*	.31168	7.7253*	.3280
Degrees of freedom	294		293		293	
Buse R^2	.9834		.9868		.9846	
Durbin—Watson	1.9730		1.9619		1.9757	
Rho	.0128		.0181		.0114	

Note: These pooled time-series estimates are obtained by estimating Kmenta's Full Cross-sectionally Correlated and Autoregressive (AR1) Model with unit effects added (Kmenta 1971, equations 12.33–40).
*$p \leq .05$.

Moving to the second and third pairs of columns of Table 4, results address the prospect that personal income tax policy may have been more responsive to low rates of investment under specific political economic conditions. To conserve space, only results for the specific post-1973 subperiods are presented. Interactions for capital investment on the one hand, and the entire post-1973 period, as well as left- and right-wing party government control on the other, are all insignificant.

However, as the table reveals, a significant effect of investment on personal income taxes is uncovered for the years 1974–79 (see %CH INVESTMENT in OPEC1). Indeed, this effect parallels the one for corporate income taxes, although (unlike the corporate income tax case) the policy effect of investment is only significant during the years of economic downturn immediately following the first OPEC oil shock. And, similar to my interpretation in the corporate income tax models, one might suggest that while recession and the advent of stagflation intensified policymaker sensitivity to the underpinnings of growth in the post-1973 era, the restrictive orientation of macroeconomic policy and burgeoning budget deficits after the second OPEC shock undercut the ideological and material bases for large tax reductions in response to lagging investment. Generally, the impact of investment in the 1974–79 period seems fairly small: a drop from 10% to 0% in the rate of capital investment results in a decline in personal income taxes of .05% of national income.

In sum, my brief exploration of the determinants of personal income tax policy has added support to the structural dependence thesis in the sense that during

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economic crisis, policymakers respond to low capital investment with reductions in personal income taxes, net of other political and economic forces shaping these tax policies. However, outside the years of the first OPEC shock, policymakers did not alter overall personal income tax burdens in response to inadequate investment. My inquiry into the determinants of personal income taxes has also shed some light on the behavior of left-wing governments: left-wing governments do indeed have positive effects on the level of personal income taxation. Together with findings from my corporate tax equations and discussion of patterns of taxation in particular countries, this result suggests that the notion that left-wing governments in capitalist democracies simultaneously pursue redistributive goals through personal income taxes (and welfare effort) and investment goals through low corporate taxes is plausible.

CONCLUSIONS

Overall, the results indicate that the generalized version of the structural dependence thesis as a theory of state behavior has merit. From my analyses, one might conclude that the state in capitalist democracies does regularly offer investment incentives and, under certain conditions, removes perceived investment disincentives in the face of inadequate capital investment. Specifically, low rates of capital investment result in reductions in corporate tax burdens, and this policy responsiveness to investment trends is more pronounced during periods of economic crisis. In addition, during periods of economic stress, low rates of investment in new productive capacity are associated with reductions in personal income taxes. In sum, these findings provide a fairly solid empirical base for a component of the structural dependence thesis that has received remarkably little empirical attention.

However, the preceding analysis also leads to qualification of a more simplistic reading of the structural dependence thesis. First, the analysis indicates that the magnitudes of the policy changes in response to low rates of capital investment are relatively small. Furthermore, my analysis of the broader political and economic forces shaping tax policy choices suggest that facets of democratic politics, features of domestic economic performance not recognized or stressed in structural dependence theory, and international economic pressures also influence taxation. Indeed, the analysis has strengthened the case for those who argue that the distinctive policy preferences of parties and patterns of party control of government are important influences on state policy choices (Castles 1982; Hibbs 1987a, 1987b; Stephens 1979). It has also strengthened the case for those who view interdependence and vulnerability to international markets as fundamental determinants of patterns of domestic policy (Cameron 1978; Katzenstein 1985).

The findings also speak to the broader question of how governments in democratic capitalism may adapt to the need to maintain adequate investment in new productive capacity and to meet pressures for policy outputs that are expressed through democratic politics. As we have seen, most, if not all, governments have regularly offered investment incentives to maintain adequate capital investment. Yet governments have choices. Left-wing governments, in particular, may confront especially important policy decisions, in that these governments have recently faced, in varying degrees, increasing challenges from the emergence of chronic economic performance problems, tax–welfare backlash movements, neoconservative macroeconomic ideas, and budget deficits. All of these forces called into question the political ability and economic capacity of left-wing governments to perform well on both the equality and growth dimensions of system performance.

As the results indicate, one strategy adopted by these governments generally is to provide relatively large reductions in the tax burdens on business to restore and maintain adequate investment in new productive capacity. From the perspective of left-wing governments within democratic capitalism, this strategy may appear to be a necessary and effective one. From the long-term point of view, maintaining ample investment incentives for business can help to secure future jobs and income for labor. From a near-term vantage point, it may obviate some of the pressure for significant retrenchments in welfare effort, progressive income taxation, and other policies. A remaining question (beyond the scope of this study) is whether a policy mix that emphasizes relatively low corporate tax burdens and generally responsive corporate tax policy within the context of high levels of personal income taxation (and welfare effort) actually works to achieve its desired policy goals.

APPENDIX

OPERATIONALIZATIONS OF THE PRINCIPAL VARIABLES INCLUDED IN ANALYSES

TAXES ON PROFITS$_t$	total taxes on corporate profits as a percentage of net operating income, time t
TAXES ON INCOME$_t$	total taxes on personal income as a percentage of national income, time t
%CH INVESTMENT$_{t-2 \text{ to } t-1}$	percentage change from t − 2 to t − 1 in real investment in machinery and equipment
LEV INVESTMENT$_{t-1}$	investment in machinery and equipment as a percentage of gross domestic product, time t − 1
PROFIT/INCOME$_{t-1}$	net operating income as a percentage of national income, time t − 1

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REAL INCOME/POP$_{t-1}$	real income per capita, time t − 1
LEFT CABINET$_{t-1}$	annual average of left-wing party cabinet portfolios as a percentage of all portfolios, time t − 1
CENTER CABINET$_{t-1}$	annual average cabinet portfolios of secular and Christian Democratic centrist parties as a percentage of all portfolios, time t − 1
RIGHT CABINET$_{t-1}$	annual average of right-wing party cabinet portfolios as a percentage of all portfolios, time t − 1
ELECTION$_{t+1}$	dummy variable for year of national legislative election, time t + 1
GROWTH$_{t-2 \text{ to } t-1}$	percentage change from t − 2 to t − 1 in real GDP
INFLATION$_{t-2 \text{ to } t-1}$	percentage change from t − 2 to t − 1 in the Consumer Price Index
OPENNESS$_{t-1}$	imports plus exports as a percentage of GDP, time t − 1
OPEC1 DUMMY$_{1974-79}$	dummy variable coded 1.00 for 1974–79; otherwise 0.00
OPEC2 DUMMY$_{1980-84}$	dummy variable coded 1.00 for 1980–1984; otherwise 0.00
AUSTRALIA . . . U.K.	15 dummy variables for countries/units (N = 16).

DATA SOURCES

Political Data

Left-wing, centrist, and right-wing party cabinet portfolios are from Eric Browne and John Dreijmanis, *Government Coalitions in Western Democracies*, (New York: Longman, 1982) and *Keesings Contemporary Archives* (selected years). Classifications of parties are based on the schema outlined in Francis Castles and Peter Mair, "Left–Right Political Scales: Some Expert Judgments," *European Journal of Political Research* 12 (1984): 73–88. For parties not covered by Castles and Mair or for ambiguous cases, supplementary information is from the *Political Handbook of the World* (selected years) and a variety of country-specific sources. Data on elections are from *International Almanac of Electoral History* (Mackie and Rose 1974); updates in *European Journal of Political Research* (Mackie and Rose selected years).

Economic Data

Data on gross domestic product, real gross domestic product, national income, net operating income, imports, exports, and capital formation are from the OECD's *National Account Statistics* (various years). Data on inflation and interest rates are from the International Monetary Fund's *International Financial Statistics* (various years). Population and labor force data are from the OECD's *Labour Force Statistics* (various years).

Policy Data

Data on personal and corporate income taxes are from the OECD's *Revenue Statistics of OECD Countries* (various years).

Notes

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1. For further expositions and critiques of the thesis, see Elkin 1985; Freeman 1989; Marsh 1983; Przeworski and Wallerstein 1988; Vogel 1983, 1989; and Ward 1987.

2. I do not purport to test all the relationships entailed by the structural dependence thesis. I do not test whether capital investment is crucial for economic growth, whether economic growth is essential for government popularity, or whether various policies are detrimental to economic performance. I simply ask whether the state behaves in the manner suggested by the thesis (as if these relationships were true or were perceived to be true) and responds to low rates of investment with policies that are favorable to the owners of capital.

3. Przeworski and Wallerstein have argued that rather than electoral support per se, some incumbent parties (e.g., left-wing parties) are motivated primarily by the desire to maximize the future welfare of a constituency other than business (e.g., labor) (1988; see also Przeworski 1990, esp. 92–96). However, to the extent that these governments believe particular policies to be a source of low investment, policy change in the favor of owners of capital will be pursued, much as under reelection-driven governments.

4. Governments may also be responsive to variations in capital investment even in the absence of adverse policy impacts; that is, as long as policymakers perceive that policies contribute to low investment, their choices may be shaped by investment considerations. Alternatively, it is possible that policymakers may simply reject the notion that certain policies necessarily engender investment disincentives. For instance, pre-1973 governments of strong Keynesian disposition (e.g., British Labour and the Swedish Social Democratic governments of the 1960s) generally adopted the view that high investment and economic growth rates were compatible with substantial income redistribution (Przeworski and Wallerstein 1982).

5. With regard to collective action, one might note that business mobilizations to act collectively in electoral and lobbying arenas may accompany declining investment and periods of economic stress and, in turn, increase policymaker responsiveness to trends in business investment (Edsall 1984; Vogel 1989).

6. This argument assumes that investment is perceived by policymakers to be relevant to relatively short-term economic performance. While Przeworski and Wallerstein question this notion (1988, 20), there is some evidence for it. For instance, the Report of the Council of Economic Advisors and other economic planning documents in the case of the United States suggest business investment is closely monitored as a determinant of short-term economic performance and that policies to counter investment downturns are readily forthcoming (Swank 1989).

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7. With respect to the welfare state, Hicks and Swank (1984b) and Pampel and Stryker (1989) explicitly test the hypothesis, inferred from structural dependence theory, that low rates of capital investment bring forth retrenchments in welfare policies because of business's preference for low levels of social welfare effort. Hicks and Swank report results consistent with the hypothesis; Pampel and Stryker do not.

8. Wallerstein (1988) extends the original analysis of Przeworski and Wallerstein (1988) to include the possibility that disinvestment may occur through international movement of capital. The results of this analysis support the earlier conclusions.

9. Although neo-Keynesian thought, which predominately influenced policymakers at least through the mid-1970s, emphasizes anticipated aggregate demand as a principal source of new investment, neo-Keynesian orthodoxy also recognizes the importance of internal finance or profits and supply-side factors (Bosworth 1984; Fazzari and Mott 1986–87). Neoconservative macroeconomic theories, ascendant in the 1970s, underscore the role of taxation by emphasizing the after-tax rate of return on investment as a key determinant of new investment in productive capacity (e.g., Fazzari and Mott 1986–87; Sahling and Akhtar 1984–85).

10. Reductions in corporate profit taxes may come through either rate reductions or tax-based investment incentives, and these two avenues have both been used across time in capitalist democracies to change corporate tax burdens (Pechman 1988). Unfortunately, cross-nationally comparable individual annual measures of corporate tax rates or investment incentives only exist for a subset of focal nations and years.

11. While recent research on the work, saving, and investment disincentives of social welfare and personal income tax policies reveals only moderate-to-weak disincentive effects of policy (e.g., Page 1983; Pechman 1990), left-wing governments faced frequent challenges to redistributive policies. Beginning in the late 1960s, tax–welfare backlash movements and parties in many countries highlighted purported disincentive effects of high levels of taxation. These trends were dramatically reinforced in the early 1970s and beyond by poor economic performance and the concomitant ascendance of neoconservative macroeconomic theories.

12. See n. 9

13. The nations are Australia, Austria, Belgium, Canada, Denmark, Finland, France, West Germany, Ireland, Italy, the Netherlands, Norway, Sweden, Switzerland, the United Kingdom, and the United States. Japan and New Zealand, two nations often included with this group, were omitted because of the absence of data on business investment in Japan before the early 1970s and large amounts of missing data for New Zealand. Iceland, Israel, Luxembourg, and several less-industrialized capitalist democracies were omitted because of nation-specific peculiarities (e.g., small size) and a paucity of relevant data.

14. Corporate tax rates generally consist of flat rates on corporate income. However, allowances and exemptions on invested retained earnings (in nearly all of the countries), lower rates on distributed profits in some nations (e.g., Austria), and low rates on initial corporate earnings in some nations (e.g., Canada) assure some cyclical association between profits and corporate tax burdens.

15. A separate control for this automatic effect is important to isolating discretionary policy response to investment. In addition, students of tax policy may also be quick to note that recent changes (i.e., post-1984) in corporate tax policy have been in the direction of eliminating investment tax credits and allowances and reducing overall corporate tax rates.

16. For the 1970s and after, this group included Australia, Canada, the Netherlands, Norway, Sweden, the United Kingdom, and the United States. Beginning in 1989, the OECD (in its *Economic Outlook*) began providing estimates of the volume indexes of private investment from the early 1970s to the present for most OECD nations.

17. The lag for %CH INVESTMENT, when the original variable is defined as change from t − 1 to t, captures changes between t − 2 and t − 1. Because many crucial tax policy choices are often made by executives in most of our nations between March and September of t − 1 and by legislatures between July and December of t − 1, this lag should tap variation in the key monitoring period of policymakers (typically, the four quarters before policy choice). Also, given the autoregressive nature of the investment variable, it should be related to past trends and short-term forecasts used by policymakers. Accordingly (and also in light of controls for automatic effects), I interpret the reported relationships between investment and taxation as reflecting discretionary policy responses. That is, such relationships may, in part, stem from legislative changes in corporate tax policy (e.g., in the mix and magnitude of investment incentives and in corporate tax rates) and discretionary action of tax authorities in enforcement and interpretation of tax codes that stem from changes in investment.

18. One might also specify the models with a lagged endogenous variable to estimate inertia and the partial adjustment of policy to investment (Johnston 1984). All focal models I discuss were reestimated with such lagged endogenous variables. These reanalyses reveal no differing conclusions for any hypotheses. (Lagged endogenous variable equation results are available on request.)

19. The first observation in generalized least squares estimation is kept by using the standard procedure offered by Poirier (1978). Examinations of nation-specific first-order rhos and residual variances suggested moderate-to-high first-order autocorrelation and low-to-moderate heteroscedasticity and contemporaneous correlation across all models presented below. To obviate concerns for any of these problems, the generalized least squares estimator was used for all models.

20. It may be helpful at this point to recall the computational procedures and interpretation of interaction terms. Briefly, the interaction of, let's say, X_1 and X_2, when the explanandum is Y will tell us whether the effect of X_2 on Y varies by levels of X_1 (or vice versa). The individual coefficient for X_2 is an index of the effect of X_2 when X_1 has a value of .00. The interaction term itself when multiplied by a given value of one of the two variables (say, X_1) and added to the coefficient of the other variable (say, X_2) becomes the slope for the effect of the second variable (X_2) at that given value of the first variable (X_1). The significance test for the interaction simply tells us whether differences in the slope of the second variable (X_2) at different levels of the first (X_1) are significantly different from zero. The significance test for the X_2 is a test of the significance of X_2 when X_1 is .00. Significance tests for effects of X_2 at any level of X_1 are readily computed. To compute the requisite standard error for the coefficient of X_2 at any given level of X_1, Friedrich (1982) points out that s_{b2-X1} is given by the square root of $[\text{VAR}(b_2) + X_1^2\text{VAR}(b_3) + 2X_1\text{COVAR}(b_2b_3]$, when b_1 and b_2 are the estimated coefficients for X_1 and X_2, respectively, and b_3 is the coefficient for the interaction $X_1 * X_2$.

21. With respect to the stability of the investment effects, one might inquire whether investment effects on corporate tax burdens are generated largely by experiences of one particular nation. Such an analysis was performed, in which the baseline model of the first pair of columns of Table 2 was reestimated 16 times, each time deleting the series for one nation. The coefficient for investment remained positive and highly significant in all cases. (Results of these 15-nation estimates are available on request.)

22. The insignificance of the capital investment variable itself in the left-wing interaction model indicates (as noted in my discussion of interaction methodology) that the effect of investment outside left-wing government, on average, is insignificant. This finding, coupled with our finding of increasingly large policy impacts of investment under right-wing governments, indicates that the policy impacts of investment under centrist governments may actually be low or nonexistent. Analysis of the tax policy effects of investment under centrist governments actually revealed that the significant, positive effect of investment on corporate tax burdens disappears in governments with more than 50% of cabinet portfolios held by centrist parties. Overall, this indicates that

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centrist parties, unlike left-wing and right-wing parties, do not systematically prefer the use of business tax relief in the face of low rates of investment, perhaps favoring fiscal and monetary policy alternatives. However, to answer this question fully and to rule out the possibility that the investment coefficient in the left-wing interaction model may have fallen below significance because of multicollinearity, a careful cross-national study of the tax policy preferences and behaviors of centrist parties over time would be required.

23. The experiences of individual nations indicate that policymakers frequently reduced tax burdens during the 1974–79 period. For instance, new investment incentives (e.g., more generous depreciation schedules and investment tax credits) or lower corporate rates were enacted in the United Kingdom (1974, 1975), the United States (1975, 1979), and France (1975, 1977, 1978). While these policies continued in the early 1980s, some nations altered course and revoked some of the recently enacted business investment incentives and corporate tax rate reductions (e.g., the United States in 1982, the United Kingdom in 1984).

24. To explore fully whether or not policymakers responded to lagging investment by reducing tax rates on those strata most likely to save, I could have used measures of tax rates by income group and examined their responses to investment. Unfortunately, cross-nationally comparable time-series data on effective income taxes by income group (e.g., deciles, quintiles) is unavailable for a large number of the focal nations.

25. Both cross-national and U.S. research has shown that the bulk of redistribution achieved through direct fiscal policies comes through income transfers (e.g., Hicks and Swank 1984a; Pechman 1990). However, personal income taxes and income transfers for our sample are highly correlated ($r = .77$, $p < .001$). Moreover, in parallel work, I have also found that left-wing party government control and left-wing party oppositional strength are related to income transfers (Hicks and Swank 1984b, 1990).

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"SOPHISTICATED" VOTING IN THE 1988 PRESIDENTIAL PRIMARIES

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Voters in multicandidate contests may confront circumstances under which it is in their interest to vote for a second- or even lower-ranked candidate. The U.S. electoral system, typically offering a choice between only two major contenders, rarely presents opportunities for this "sophisticated" voting. In presidential primaries, however, many plausible candidates may compete. We investigate the presence of sophisticated voting in the 1988 presidential primaries, using data from the National Election Study's Super Tuesday survey. We examine patterns of voting types based on ordinal measures of preferences among candidates and assessments of their chances of winning their party's nomination and estimate several models of choice, testing the multicandidate calculus of voting. Among both Republicans and Democrats, respondents' choices were consistent with the calculus of voting and thus with sophisticated voting.

Voters make decisions in a world of imperfect information, but the lack of information is particularly acute in nomination contests. Voters cannot rely upon their partisan loyalties to reduce information costs, and they often must choose among candidates with similar policy positions. Moreover, unlike most general elections, in which the effective choice is between two major party candidates, nomination contests often involve three or more candidates. The choice among candidates is particularly complicated in U.S. presidential nominations because, unlike most party nominations, choices are not made in a single primary election. Instead, the nomination contest lasts for several months; and the information available to the primary or caucus voter varies greatly over time.

As presidential nominations are usually multicandidate contests, voters may confront situations in which they have reasons not to vote for their most preferred candidate. At the outset of the campaign, voters may have very little information about the candidates' prospects. After the Iowa caucuses and the New Hampshire primary, some candidates are winnowed out; others continue with substantially reduced prospects; and still others have greatly enhanced chances of victory. New information about the candidates' prospects may lead some voters to alter their choices. While some may support their most preferred candidates regardless of their prospects of victory, others may also weigh those prospects. Those who vote for their most preferred candidate without taking that candidate's chances into account cast a *sincere vote*. Those who weigh both their preferences and the candidates' chances cast a *strategic, or sophisticated, vote*, perhaps voting for their next-most-preferred—or an even lower rated—candidate.

Many popular accounts of presidential nomination contests describe the electorate as ill informed, capricious, and vulnerable to media manipulation. Some academic accounts are equally unflattering (e.g., Keeter and Zukin, 1983). Strategic or sophisticated voting (or any other form of rational voting) seem at odds with such accounts.

Our analysis suggests that voters make relatively good use of the available information. Voters appear to have relatively stable evaluations of the candidates, and they appear to be attuned to their fortunes in a reasonably informed and sensible way. Most voters do support the candidate they most prefer; but many appear to be guided in part by information about the candidates' prospects of winning their party's nomination, or *viability*.[1] That voters often act strategically tends to support rational choice models of behavior, particularly the "calculus of voting" (Downs, 1957; Riker and Ordeshook, 1968, 1973) that combines utility and probability terms. Such behavior is inconsistent with Ferejohn and Fiorina's minimax-regret model (1974, 1975), which predicts that rational voters never engage in strategic voting.

The 1988 presidential contest provides an excellent opportunity to test for the presence of sophisticated voting. Most importantly, it is the first contest since the post-1968 reforms in which neither party had an incumbent president seeking his party's nomination. As a result, both contests began with many major contenders. Moreover, the National Election Study's (NES's) Super Tuesday survey, conducted by the Center for Political Studies of the University of Michigan, provides appropriate data for studying individual-level choices.

This study was conducted using telephone polling from 17 January through 8 March (Super Tuesday) in the 16 states holding primaries on Super Tuesday. A total of 2,117 respondents were interviewed, and the continuous monitoring design provides a random sample for individual weeks or other time periods.[2] The survey asked respondents which Democratic and which Republican candidate they would vote for—our dependent measure, *vote preference*.[3] Two other

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questions provide the information needed to ascertain whether the vote preference was strategic, sincere, or of some other type. First, respondents were asked to evaluate all candidates they had heard of on a feeling thermometer (0–100 degrees in ascending favorability). Second, respondents were asked to rate each of these candidates' chances of winning the Democratic or Republican presidential nomination on another hundred-point scale.

The Super Tuesday Study suggests that voters may have been behaving in a sophisticated manner. Among Republicans, George Bush held a 37-point lead in vote preferences over Robert Dole in the weeks before the Iowa caucuses (see Table 1).[4] After the Iowa caucuses (8 February), in which Dole finished first and Bush finished third, Dole jumped ahead by 8 points. But on 16 February, Bush won the New Hampshire primary, with Dole finishing second. Bush held a 29-point lead among Republicans interviewed after the New Hampshire primary. But during these time periods candidate evaluations, as measured by the feeling thermometers, changed very little. There were, however, substantial changes in their perceived viabilities. The relative stability of evaluations suggests that the dramatic changes in voter preferences were due to the equally dramatic changes in viabilities, a pattern consistent with the theory of strategic voting.

The table also provides evidence of sophisticated voting among Democrats. Before the Iowa caucuses, Dukakis held a 10-point lead over Gephardt in both voting preferences and viability, along with a slightly smaller lead in candidate evaluations. After Gephardt's first-place showing in Iowa, with Dukakis finishing third, Gephardt emerged with an 11-point lead in voting preferences, a 4-point edge in evaluations, and a 7-point advantage in perceived viability, although Dukakis led in Gallup polls conducted in New Hampshire (see Abramson, Aldrich, and Rohde, 1991, table 1.2). After Dukakis' New Hampshire win (with Gephardt finishing second), Dukakis regained his lead on all three measures; but his gains in vote preference and perceived viability were much greater than his gains on the candidate thermometers.

This aggregate evidence suggests that voters may be responding to changing information, and may condition their choice of candidates both on their evaluations of the candidates and on their perceptions of the candidates' chances for success. Before analyzing these data further, we will briefly review the multicandidate calculus of voting, the theory of sophisticated voting.

SOPHISTICATED VOTING AND THE THEORY OF THE CALCULUS OF VOTING

The logic of not "wasting" a vote on a hopeless candidate or party has long been discussed in popular and academic accounts of elections; but this logic applies only when there are more than two choices. In most U.S. general elections, third-party candidates are so weak that few voters even consider voting for them. But when third-party candidates generate support, as in the 1980 presidential election, arguments about wasting votes are likely to be raised and acted upon (see Abramson, Aldrich, and Rohde, 1983, 174–76).

The wasted vote logic is compatible with the theory of the calculus of voting, which is based on expected utility maximization. Originally developed by Downs (1957) and Riker and Ordeshook (1968, 1973) to model turnout in two-candidate races, the calculus was extended to multicandidate contests and conditions for sophisticated voting were derived by McKelvey and Ordeshook (1972). Black (1978) and Cain (1978) tested versions of this calculus using Canadian and British survey data, respectively, and found strong evidence that some voters cast sophisticated votes.

The essential logic is simple. Suppose there are three candidates—A, B, and C—whom a voter prefers in that order. Suppose that A has very little chance of winning, but that the race between B and C is close. One form of sophisticated voting occurs if the voter casts a ballot for B to avoid wasting a vote on A's hopeless candidacy. The keys to the calculus of voting are the relative intensity of preferences (utility comparisons) for the three candidates and the voter's perception that his or her vote can help make a candidate win or lose. The keys, that is, are the *expected utilities* associated with voting for A, B, or C or with abstaining.

As can be demonstrated (see Abramson et al. 1990; Aldrich 1991), the rational voter's choice depends on the comparative *utilities* associated with the candidates and the relative *probabilities* of outcomes. Thus, the likelihood of voting for the second-choice candidate increases as the voter evaluates A and B more similarly and as B is increasingly preferred to C. It also increases as the chances that one's vote would help B win grow and as the chances of A's winning decline. For the sophisticated voter, the appropriate action depends on the relative comparisons of utilities and probabilities.

Because the probability of a single vote affecting the outcome of an election is very small, some argue that very few voters will consider the candidates' chances (see Barry 1970; Ferejohn and Fiorina 1974, 1975). Admittedly, in the real world, very few voters think in terms of making or breaking a tie. Like all theories, the calculus of voting is a simplification of reality that seeks to capture the most salient features of actual situations. Many voters may see some candidates as having real chances of winning and others as likely losers, and they may weigh these perceptions against the relative attractiveness of the candidates. This might be especially true in presidential primaries; for each vote has a small, but real, chance of increasing a candidate's delegate total. Moreover, especially in the early stages of the primary season, momentumlike

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TABLE 1.

Vote Preferences, Candidate Evaluations, and Perceived Nomination Prospects of Candidates by Party and Date of Survey

CANDIDATES AS PERCEIVED BY SUPPORTERS OF THEIR OWN PARTY	TIME PERIODS: PRE-IOWA CAUCUSES	IOWA CAUCUSES TO NEW HAMPSHIRE PRIMARY	NEW HAMPSHIRE PRIMARY TO SUPER TUESDAY PRIMARIES
Percentage vote preference			
Bush	60.0	39.4	56.7
Dole	23.5	47.4	28.2
Difference	36.5	−8.0	28.5
	(230)	(137)	(305)
Average thermometer score			
Bush	69.5	66.2	70.5
	(223)	(131)	(297)
Dole	66.2	68.5	66.7
	(183)	(124)	(264)
Difference	3.4	−2.3	3.8
Average chances of nomination			
Bush	76.6	67.6	75.0
	(223)	(130)	(297)
Dole	64.0	71.7	67.5
	(183)	(125)	(265)
Difference	12.6	−4.2	7.5
Percentage vote preference			
Dukakis	18.9	20.2	34.6
Gephardt	9.0	31.2	15.4
Difference	9.9	−11.0	19.1
	(212)	(109)	(324)
Average thermometer score			
Dukakis	60.5	58.9	61.3
	(120)	(69)	(225)
Gephardt	54.5	63.2	56.1
	(105)	(74)	(214)
Difference	6.1	−4.3	5.3
Average chances of nomination			
Dukakis	57.2	59.8	68.3
	(120)	(70)	(224)
Gephardt	47.1	66.7	57.8
	(107)	(74)	(213)
Difference	10.2	−6.9	10.5

Note: Number of cases in parentheses.

dynamics may magnify the impact of each additional delegate (see Aldrich 1980a, 1980b; Bartels 1988).

Sophisticated voters may wind up voting for their first choice. A sophisticated vote is one based on weighing both preferences and probabilities of success. A sincere vote is based upon preferences alone and leads to voting for the most preferred candidate, regardless of circumstances. When a voter's sophisticated and sincere choices coincide, the choice is called "straightforward" (Farquharson 1969). That most citizens vote for their first-ranked alternative, therefore, does not demonstrate that they are voting sincerely. Many may be sophisticated voters facing circumstances that yield a straightforward choice.

THE CAMPAIGN SETTING

The 1988 presidential nominations were contested vigorously by six major Republican candidates and

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seven Democrats. As in all contests since 1976, the Iowa caucuses and the New Hampshire primary were the two most important early contests. Three weeks after the New Hampshire primary, 20 states, concentrated in the South, held primaries or caucuses on the same day. Super Tuesday provided by far the largest number of delegates to be won on a single date and became the third important campaign event. Bush had been the Republican front-runner from the beginning, with Dole seen as his major challenger. After the Iowa caucuses, Dole led Bush in polls conducted in New Hampshire (see Abramson, Aldrich, and Rohde 1991, table 1.2), much as he did in the Super Tuesday survey (see Table 1). But after his New Hampshire victory, Bush was again the front-runner. Alexander Haig had withdrawn four days before the New Hampshire primary, and Pierre ("Pete") du Pont IV withdrew two days after that primary. Jack F. Kemp, Pat Robertson, and Dole had all been seriously weakened.

The Democratic race was more complicated. No candidate could be considered a clear front-runner, although both Gephardt and Dukakis were major candidates. Among the seven Democrats, only Bruce Babbitt (who quit two days after the New Hampshire primary) withdrew before Super Tuesday. But Iowa and New Hampshire had dramatically altered the prospects of several candidates. Gary Hart's chances were drastically reduced, and Paul Simon announced that he would sit out Super Tuesday. Gephardt came in second in New Hampshire; but he trailed Dukakis by 16 percentage points, diminishing his prospects. On the other hand, Jesse Jackson and Albert Gore, Jr. had yet to make a major effort; and Super Tuesday would be their first major test.

These events apparently led voters to reevaluate the prospects of the competing candidates, and these changed perceptions may have affected voting preferences. However, supporters of Robertson and Jackson might be less affected than the supporters of other candidates. From the outset Robertson and Jackson were given little chance of winning, but their supporters were strongly committed to them. Gore's supporters might believe that his prospects were yet to be tested. One might expect, therefore, high levels of sincere voting for Robertson, Jackson, and perhaps Gore. Such expectations, however, could also be consistent with sophisticated voting, yielding straightforward choices based on strong preferences.

Our preliminary analysis of the Super Tuesday Study suggests that there was sophisticated voting; but a better test requires examining individual-level preferences, evaluations, and assessments of the candidates' chances of winning. As noted, we have used the hundred-point feeling thermometers to create both ordinal preference rankings of the candidates and interval scores. For the latter, the hundred points is an arbitrary absolute scale. These scores can be viewed as measuring the utility the respondent attaches to electing each candidate. Theoretically important are the intervals between the various candidates. Without loss, we can assign a score of one to the highest-ranking and zero to the lowest-ranking candidate (say, candidate n). What is critical, then, is where the middle-ranked candidates fall. For example, the score for the second-ranked candidate can be normalized as follows:

$$u_2 = (U_2 - U_n)/(U_1 - U_n), \qquad (1)$$

where U_j indicates the original thermometer score and u_2 indicates the "normalized" evaluation score for the second-ranked candidate.[5] Hence, for all respondents, the normalized scores are one for the highest-ranked candidate, u_2 for the second-ranked candidate, and zero for the lowest-ranked candidate. Thus, u_2 will be between zero (if candidate 2 is tied in score for lowest-ranking) and one (if two are tied for top-ranking). Normalized scores can be derived for all other middle-ranked candidates in similar fashion.

Assessments of candidates' chances of winning nomination were also measured by hundred point scales, and these responses were also normalized. If n candidates are rated, let p_j denote the "normalized" probability score and P_j denote the original score for candidate j. Then, "normalized" viability scores can be found by

$$p_j = P_j/(P_1 + \ldots + P_n). \qquad (2)$$

These variables provide the basic data for estimation.

PREFERENCES, EVALUATIONS, AND VIABILITIES

We begin by examining the relationship between voter preferences and candidate evaluations and between voting preferences and viability ratings. Among the 649 Republicans with a vote preference who rated a candidate, 87% had a vote preference for the candidate they rated highest on the thermometer scale. Among the 609 Democrats with a vote preference and who rated at least one candidate on the feeling thermometers, 85% reported a vote preference for the candidate they rated highest on the thermometer scale (for detailed results, see Abramson et al. 1990, table 2). As high as these percentages are, they are lower than the 95% who report voting for their highest-ranked major party candidate in NES general election surveys (see Abramson, Aldrich, and Rohde 1983, table 8.7). Ten percent of the Republicans and 9% of the Democrats registered a vote preference for their second-ranked candidate. These figures do not demonstrate that the vast majority of respondents are sincere. Many may have strategically straightforward choices.

Among the 647 Republicans with a vote preference who ranked a candidate on the hundred point viability scale, 79% reported a vote preference for the candidate they thought most likely to win the party's nomination; among the 608 comparable Democrats, 72% reported a vote preference for the candidate they thought most viable. (For detailed results, see

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TABLE 2.

Classification of Voting Types Based on Rank Orders of Evaluations and Perceived Viability

EVALUATION RATING OF RESPONDENT'S CHOSEN CANDIDATE	RANKING OF NOMINATION VIABILITY OF RESPONDENT'S CHOSEN CANDIDATE		
	HIGHER	SAME	LOWER
HIGHER	straightforward by evaluations and viability	straightforward by evaluations	apparently sincere
SAME	apparently sophisticated by viability	straightforward by equality	irrational by viability
LOWER	apparently sophisticated	irrational by evaluations	irrational by evaluations and viability

Note: One-paired comparison. Four other categories are (1) rating exactly two candidates on feeling thermometer, with vote preference for the higher-rated candidate, classified as *rational but uninteresting*, since sincere and sophisticated choices are indistinguishable; (2) rating only one candidate on the feeling thermometer, with vote preference for that candidate, classified as *rational but uninteresting*; (3) having vote preference for candidate rated lowest overall on feeling thermometer, classified as *irrational*, and (4) having vote preference for candidate not rated on feeling thermometer, classified separately (i.e., neither rational nor irrational).

Abramson et al. 1990, table 3. In each party, 14% had a voting preference for the candidate they believed was the second-most-likely to win the party's nomination.) Once again, this does not necessarily indicate that most respondents were purely sincere. They, too, might have straightforward choices. Indeed, these data suggest that many supported the candidate they liked best and believed had the best chances of success.

To some extent voters may overestimate the chances of the candidates they like, and some may like candidates they believe will be successful. Bartels (1988) examined the relationship between expectations and preferences, finding that both Mondale's and Hart's thermometer evaluations in the 1984 primary season were a function, in part, of respondents' perceptions of their chances of winning the nomination and also that these perceived chances were, in part, a function of respondents' evaluations on the feeling thermometer. He also found similar relationships in data from the 1980 nomination contests (Bartels 1985). Paolino (1989) reported similar findings with 1988 data. Our own analyses, reported below, suggest that candidate evaluations affect assessments of viability. Thus, it appears that respondents partially project their evaluations of candidates onto their perceptions of candidate viability, seeing candidates they evaluate highly as more likely to be nominated. According to our estimates, respondents do not appear to rationalize their evaluations of the candidates.

Because respondents are likely to overestimate the chances of their preferred candidates, the number of seemingly straightforward voters may be inflated. We believe this would be true mainly for the most highly evaluated candidate. Viabilities would not, it seems likely, be projected consistently across the sample for candidates rated second-highest. Thus, the extent of apparent straightforwardness, that is, voting for one's first choice who also is considered most viable, might be higher in these data than it "should" be; but voting for second-choice candidates should be largely unaffected. We shall return to this point later.

ANALYSIS OF ORDINAL VOTER TYPES

The critical question is how voting preferences are affected by the combination of preferences and expectations about viability. We shall examine these relationships in an ordinal framework before turning to model estimation. The logic can be seen in Table 2, which presents how a respondent can compare two candidates' ordinal evaluations and viabilities. Our full classification is based upon the comparison between the candidate the respondent prefers to support and every other candidate. The resulting typology is reported in Table 2.

The cell in the upper right-hand corner of Table 2 corresponds to a respondent whose vote preference is for the candidate ranked higher on the feeling thermometer but thought to have lower viability. This configuration is the classic case of sincere voting, that is, voting for the less viable but more preferred alternative. We call this type of voter *apparently sincere*, because actual sincerity depends upon the order of magnitude involved. The upper left-hand cell typifies the *straightforward* voter. For this voter, there is no trade-off between evaluations and viability, since the highest-rated candidate is also the most viable one. The bottom left-hand cell corresponds to *apparently sophisticated* voting, because the individual supports a more viable candidate over one who is better liked. The bottom right-hand cell is labeled *irrational*, since the voter supports a candidate who is less well liked and less viable than another. A complete typology of response types includes nine additional cases. Five types involve ties and are displayed in Table 2. Four additional categories are listed below the table.

This typology of voting types is developed by examining all pairs of candidate comparisons, using only ordinal information. (See Abramson et al. 1990,

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TABLE 3.

Distribution of Voting Types by Political Party (%)

VOTING TYPE	POLITICAL PARTY	
	REPUBLICANS	DEMOCRATS
Straightforward	52.8	43.8
Apparently sophisticated	13.9	13.1
Apparently sincere	10.3	14.3
Rational but uninteresting	6.5	10.5
"Irrational"	10.0	11.7
Vote preference for unrated candidate	4.6	6.8
Total Percent	100.1%	100.2%
Number of cases	680	650

app. 1, for a detailed explanation.) The candidate the respondent preferred to vote for was compared to every other contender for the party's nomination in turn. If the preferred candidate was rated higher than every other candidate on both evaluations and viability, the respondent was classified as straightforward; and so on.

The basic distribution of these typologies is reported in Table 3. In both parties, straightforward voting was the most common classification. About one in seven was classified as apparently sophisticated, while a few more Democrats and a few less Republicans were apparently sincere. Between 10 and 12% were classified as irrational and 6 to 10% as rational but uninteresting. The remaining 5 to 7% preferred a candidate whom they did not rate on the feeling thermometer.

Table 3 suggests that most of the action is concentrated in the three most interesting cases for our analysis: the straightforward, apparently sincere, and apparently sophisticated categories.[6] Counting rational but uninteresting cases, over 8 in 10 supported a candidate in a fashion consistent with rational choice. Most respondents did not to have to face a tough choice between a candidate they liked most and another they believed more likely to succeed. When there was such a trade-off, respondents were somewhat more likely to support the candidate they thought would win.

We can use these classifications to examine some political questions. Do these patterns change over time, due either to the changing circumstances of the campaign or simply to the proximity of the interview to Super Tuesday? Is there more apparently sophisticated reasoning among some subgroups than others? The relative sizes of the categories of *uninteresting, irrational,* and *preferring an unrated candidate* might be affected by the level of information held, assimilated, and used by a respondent. Thus, we might expect the frequency of these categories to decrease over time and/or as the political and intellectual skills and attributes of the respondents increase. The *uninteresting* category, for example, may become less common as the primary day nears, simply because more respondents have more information and therefore rate more candidates. The use of more complex information in decisionmaking may increase with either time or awareness. For example, the incidence of apparently sophisticated choices might increase, relative to apparent sincerity.

Our analysis reveals that there were relatively few differences among voting types between Republicans and Democrats (see Table 3) by the time period of the survey, level of education, interest in the campaign, or extent to which the respondent followed the campaign in the newspapers (see Abramson et al. 1990, tables 5–8).[7] To be sure, increasing information and proximity to Super Tuesday do lead to fuller and richer evaluations; and, as expected the proportion in the *rational but uninteresting* and *vote preference for unrated candidate* categories decline as political involvement increases. There is little in these data to suggest, however, that voting straightforwardly, strategically, or sincerely varies substantially. In short, fuller information leads to rating more candidates but does not seem to affect greatly how these evaluations and assessments of viability are employed in making decisions.

Vote preferences, by contrast, are strongly related to voting types, as the results in Table 4 reveal. It reports the distribution of voting types among Republicans who preferred Bush, Dole, and Robertson. Those who preferred Robertson were far more likely to be classified as apparently sincere than were those who preferred Bush or Dole. Conversely, very few of Robertson's supporters were apparently sophisticated voters. Those who preferred Bush present the opposite profile. He was the straightforward choice for three out of five of his supporters. Most of the remainder came to prefer him as the apparently sophisticated choice, while very few were apparently sincere. Supporters of Dole fell somewhere between those preferring to vote for Bush or Robertson, but they were more similar to Bush supporters.

The table shows a similar pattern among supporters of the five leading Democrats. Those who preferred Jackson were much like Republicans who preferred Robertson—disproportionately likely to be apparently sincere and very unlikely to be apparently sophisticated. The reason is the same in both cases—

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TABLE 4.

Distribution of Voting Types by Vote Preference (%)

	REPUBLICANS			DEMOCRATS				
VOTING TYPES	BUSH	DOLE	ROBERTSON	DUKAKIS	GEPHARDT	GORE	HART	JACKSON
Straightforward	60.3	51.9	32.8	54.6	51.0	50.5	36.8	33.3
Apparently sophisticated	19.2	15.9	1.6	23.9	18.3	7.1	10.4	2.9
Apparently sincere	2.2	12.0	32.8	5.1	5.8	17.2	14.9	21.7
Rational but uninteresting	9.7	1.0	9.4	3.9	3.9	8.1	21.8	21.0
"Irrational"	5.1	12.5	20.3	5.7	9.6	12.1	10.4	15.9
Vote preference for unrated candidate	3.5	6.7	3.1	6.8	11.5	5.0	5.8	5.1
Total Percent	100.0%	100.0%	100.0%	100.0%	100.1%	100.0%	100.1%	99.9%
Number of cases	370	208	64	176	104	99	87	138

few thought their preferred candidate had a good chance of winning. Those who preferred Dukakis were much like those who preferred Bush, with many falling into the straightforward and into the apparently sophisticated categories. Few were categorized as apparently sincere. Gephardt supporters were almost as likely as Dukakis supporters to be classified as straightforward and as sophisticated voters. Half of Gore's supporters were straightforward, as well; but they were less likely to be apparently sophisticated and more likely to be apparently sincere.

We have reported elsewhere the distribution of voting types by candidate preferences in the three major time periods of the campaign (see Abramson et al. 1990, table 2A5). We expect, for example, that after his loss in Iowa, fewer Bush supporters should be apparently sophisticated and more might be apparently sincere or straightforward by preferences alone. We expect these patterns to be reversed after his New Hampshire victory. Those preferring Dole should reveal the opposite patterns of changes. Patterns of voting types among Democrats preferring Dukakis should be similar to those among Republicans preferring Bush, while Democrats preferring Gephardt should be similar to those preferring Dole. There should be relatively little change in voting types among those Robertson or Jackson supporters, since they tended to prefer their candidate regardless of his chances.

The over-time changes among those preferring to vote for Bush and Dole are uniformly consistent with these expectations; and many of these comparisons, especially in the *straightforward by evaluations* subcategory, are large. Among Democrats, these patterns are mostly consistent with these expectations, as well, although the magnitudes are often somewhat smaller.

These analyses suggest two major conclusions: (1) the distribution of voting types varies little with measures of political involvement; and (2) straightforward, apparently sincere, and apparently sophisticated voting types do vary substantially by respondents' preferred candidates. Moreover, these types vary over time in ways that are consistent with the realities of the campaign. Thus, this analysis suggests that respondents are attuned to the chances of the candidates and incorporate such considerations in their candidate preferences.

ESTIMATION OF MODELS BASED ON THE MULTICANDIDATE CALCULUS OF VOTING

The multicandidate calculus of voting assumes that all voters are "sophisticated," because it assumes that all choose on the basis of expected utility maximization. The central terms in the determination of voters' choices, therefore, involve their candidate preferences (or utilities) and their assessments of the candidates' viabilities. We shall test two statistical models based on the calculus of voting.

We begin by examining viability, given candidate preference orders—replicating analyses conducted by Black (1978), who used data from the 1968 and 1972 Canadian federal elections. He employed ordinal preference rankings from survey responses and measures of probability terms based on actual election returns. His tests were based on the assumption of the calculus of voting that voters who shared similar preference orderings would be more or less likely to vote for their most- or next-most-preferred party, given differing probabilities of casting decisive votes.

Here, we estimated similar models. The dependent variable is dichotomous, indicating whether respondents preferred to vote for their first (scored one) or second (scored zero) most highly ranked candidate, as measured by the feeling thermometers. As we reported above, about 85% preferred to vote for their first-ranked candidate. Ten percent preferred to vote for their second-ranked candidate, a result similar to the percentage that reported voting for their second-ranked party in Cain's (1978) study of the 1970 British general election. Too few preferred any lower-rated candidate to make estimation feasible.

The independent variables are comparisons of the

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TABLE 5.

Probit Estimates of the Probability of Preferring to Vote for the First- or Second-highest-ranked Candidate, Given Ordinal Preferences and Differing Assessments of Candidate Viability

INDEPENDENT VARIABLES	MLE		
	REPUBLICANS	DEMOCRATS	BOTH PARTIES
P_{12}	2.285* (.600)	3.123* (.610)	2.715* (.421)
P_{13}	1.707* (.546)	1.290* (.529)	1.413* (.370)
Constant	1.040* (.0915)	1.194* (.0884)	1.121* (.0623)
−2xLLF (χ^2_2)	44.68*	69.40*	111.99*
Number of cases	503	483	986

Note: MLE denotes maximum likelihood estimate. P_{xy} denotes the difference in normalized viability scores given to candidates x and y. LLF denotes Log of Likelihood Function. Standard errors are in parentheses.
* $p \leq .05$ (one-tailed test of coefficient estimates).

assessments of viabilities. (Differences in the normalized viability measures [see equation 2] will be used and referred to in Tables 5 and 6 as P_{12} for the difference in viability between the *first-* and *second-*rated candidates, and so on.) The large number of paired comparisons of viabilities, due to the large number of candidates, prohibit including all possible paired comparisons.[8] Therefore, we have included comparisons among the candidates rated highest and next highest on the feeling thermometers and the candidate the respondent assesses as most viable of the remaining, lower-rated contenders. Since the hypothesis of sophisticated voting is that voters consider supporting their second-choice candidate to avoid selection of some even lower-rated candidate, this procedure should select the lower-rated candidate whom voters see as the most significant "threat." As a result, there are three viability comparisons, P_{12}, P_{13}, and P_{23}. Parameter estimates of the first two variables should be positive, while the third should be negative. Note that by definition, $P_{23} = P_{13} - P_{12}$. Therefore, only the later two variables are included in the estimation.[9]

The results of probit estimates of this model are reported in Table 5 for Republicans, Democrats, and the two parties combined. The overall fit of these models is strong and statistically significant. The viability difference variables are significant and correctly signed in all three cases. The coefficient estimates are relatively large, and their impact can be consequential. For example, compare two voters with normalized viability estimates of their three candidates of .5, .3, and .2 and of .3, .5, and .2, respectively. Based on the Republican estimates, the first voter would be expected to vote for the most preferred candidate with a probability of .98, while the second voter's likelihood of voting for the most preferred candidate drops to .77. The comparable Democratic probability estimates are .99 and .76, respectively. In short, the estimates clearly support the theory; the similarity in coefficient estimates between the two parties is striking; and the impact of the coefficients is substantial.

A second set of probit models more fully exploits the measures available. The heart of expected utility is the multiplication of utility and probability; and we construct such models here by multiplying the difference in viability of two candidates by the difference in utility evaluations (see equation 1). Cain (1978) estimated similar models using data from the Butler and Stokes survey of the 1970 British general election. The dependent variable is the same as in Table 5, and the same sets of three candidates are employed. The independent variables are denoted as PB_{jk} in the tables, representing $(P_j - P_k)(U_j - U_k)$ for candidates j and k, and the normalized measures of both viability and utility are used (see equations 1 and 2). As before, we expect coefficient estimates for the PB_{12} and PB_{13} to be positive, since greater expected utility for the most preferred candidate compared to the other two should make supporting the most preferred candidate more likely. Conversely, we expect the parameter estimate for the PB_{23} variable to be negative, controlling for the other two variables. As the expected utility for the second candidate is increasingly larger compared to the third candidate, the likelihood of voting for the second-most-preferred candidate should increase. In this case, PB_{23} is not, by definition, equal to the difference in the other two variables; therefore, all three variables can be included.

The probit estimates for these models are reported in Table 6. The overall goodness-of-fit statistics are again large and significant for all three estimations (Republicans, Democrats, and both parties combined). All parameter estimates have the correct sign, and those involving the most preferred candidate are statistically significant in each model. The PB_{23} coefficient is large and statistically significant for Democrats and for both parties, although it is small and insignificant (albeit correctly signed) for Republicans. As in the first model, the impact of the independent

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TABLE 6.

Probit Estimates of the Probability of Preferring to Vote for the First- or Second-highest-ranked Candidate, Given Normalized, Multiplicative Utility and Viability Measures

INDEPENDENT VARIABLES	MLE		
	REPUBLICANS	DEMOCRATS	BOTH PARTIES
PB_{12}	5.807*	5.335*	5.347*
	(2.479)	(1.746)	(1.383)
PB_{13}	2.188*	3.650*	2.932*
	(1.028)	(.911)	(.647)
PB_{23}	−.567	−3.205*	−1.988*
	(1.289)	(1.182)	(.828)
Constant	1.029*	1.151*	1.087*
	(.0900)	(.0885)	(.0618)
−2xLLF (χ^2_3)	33.03*	71.10*	100.47*
Number of cases	495	475	970

Note: MLE denotes maximum likelihood estimate. PB_{xy} denotes the difference in normalized viability scores times the difference in normalized thermometer evaluation scores given to candidates x and y. LLF denotes Log of Likelihood Function. Standard errors are in parentheses.
* $p \leq .05$ (one-tailed test of coefficient estimates).

variables can be substantial. For example, if we repeat our example, but with normalized utilities of (1, .7, 0) for the three candidates, the estimates reported in Table 6 yield likelihoods of voting for the highest-rated candidate of .98 and .99 for Republicans and Democrats, respectively, when the three viabilities are (.5, .3, .2). These likelihoods decline to .78 and .70, respectively, with viabilities of (.3, .5, .2). If the utility for the second-rated candidate is .3, the estimated likelihoods are .99 and .999, respectively, for the first set of viabilities and .69 and .68 for Republicans and Democrats for the second set of viabilities. These examples are not extreme, yet the model estimates predict substantial differences in the probability of voting for the first- versus second-rated candidate.[10]

SOME ADDITIONAL EVIDENCE

Differences among Supporters of Different Candidates

We shall explore two further questions that, while not direct tests of the calculus of voting, are important to this study. One set of model estimates examines the possibility that supporters of different candidates employed different decision rules. In particular, the data in Table 4 suggested that Robertson and Jackson supporters might have been sincere, ignoring the viability assessments. Alternatively, they may have been sophisticated voters who so greatly favored their first choice over the alternatives that they had straightforward sophisticated strategies. The second question concerns estimating the extent and consequences of any rationalization of thermometer or viability ratings.

The first question requires us to examine candidate choice by name rather than their position in preference order. Moreover, testing whether supporters of some candidates employed viability assessments in their decisionmaking but others did not requires us to examine the impact of viability and evaluations separately, rather than multiplicatively as in our tests of the calculus of voting. While not a direct test of the calculus of voting, therefore, the results will determine the robustness of our estimates across differing types of candidates and campaigns.

The dependent variable is the candidate the respondents preferred. This is a purely categorical variable in both parties, examining Bush, Dole, and Robertson among Republicans and Dukakis, Gephardt, Gore, and Jackson among Democrats.[11] Therefore, multinomial logit was used for estimation. Since scale effects could be material, the direct, rather than normalized, measures of evaluations and viabilities are used. Thus, the independent variables are the individual candidate feeling thermometer scores and the individual assessments of each candidate's viability. With k candidates, multinomial logit estimates $k - 1$ equations, with the remaining candidate serving as the baseline. Coefficients, therefore, indicate the effect of a variable on increasing the likelihood that the respondent will support the indicated candidate over the baseline candidate. The probability of preferring the baseline candidates (here Bush and Dukakis) is one minus the probability of supporting all other candidates.

The estimates are reported in Tables 7 and 8. The goodness-of-fit statistic for each party is very large. There is also general support for the major hypothesis that vote preference depends on evaluations and

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TABLE 7.

Multinomial Logit Estimates of Republican Vote Preference, Given Candidate Evaluations and Assessments of Candidates' Viability

	DOLE	ROBERTSON
Bush		
Evaluation	−.0979*	−.1113*
	(.0133)	(.0189)
Viability	−.0658*	−.0603*
	(.0131)	(.0207)
Dole		
Evaluation	.1068*	−.0509*
	(.0145)	(.0214)
Viability	.0514*	.0470*
	(.0129)	(.0237)
Robertson		
Evaluation	−.00376	.1608*
	(.00747)	(.0256)
Viability	−.000976	.0276
	(.00826)	(.0184)
Constant	.4825	−1.9007
	(.9135)	(1.7344)
−2xLLF (χ^2_{12})	503.33*	
Number of cases	482	

Note: Bush serves as baseline candidate. LLF denotes Log of Likelihood Function. Standard errors are in parentheses.
* $p \leq .025$ (one-tailed test), $\leq .05$ (two-tailed test).

viability. There are some small differences between the two parties.

The Bush–Dole comparisons are just as expected. The higher the evaluations and the viability assessments of Bush, the more likely the respondent to report a vote preference for Bush; and the higher the scores on these two variables for Dole, the more likely the respondent to support Dole. Evaluations of all three candidates were significantly related to reporting a vote preference for Robertson, although Dole evaluations are most weakly so, as expected. Viability assessments of Robertson were positively, but only weakly and not significantly, related to his support. This suggests sincerity among his supporters; but those with a Robertson vote preference did weigh his opponents' viabilities, Bush negatively and Dole positively. Thus, Robertson supporters were not purely sincere, but weighed his opponents' prospects, consistent with the calculus of voting.

Estimates of the model of Democrats' vote preferences are clear; and there is general support for the hypothesis that supporters of all candidates followed the logic of the calculus of voting, weighing both preferences and viability. The probability of expressing a vote preference for Gephardt and for Gore, compared to Dukakis, fit the expected pattern very closely. The higher the evaluation or assessment of viability for Dukakis, the less likely respondents were to express a vote preference for Gephardt or for Gore. Conversely, the more positively Gephardt or Gore were evaluated and the candidate's viability assessed, the more likely the respondent was to have a vote preference for Gephardt or Gore, respectively. The positive signs on evaluations of Gore and Gephardt in each other's equations suggest that their supporters saw them as near substitutes, with Dukakis as the main alternative.

The comparison between Jackson and Dukakis is generally consistent with the expected utility logic. Evaluations of Jackson and Dukakis perform as expected. The more viable Jackson was seen to be, the more likely respondents were to express a vote preference for him. While the opposite is true for assessments of Dukakis' viability, as expected, the parameter estimate is not significant at the conventional .05 level, although it is nearly so (and is significant at the .1 level). Thus, the conclusion is that the Jackson supporters appeared to reason via expected utility but that the strength of that model, especially in terms of the viability coefficients, is somewhat less. In this sense, there is a reasonable parallel between Robertson and Jackson supporters, where viability assessments are consequential but somewhat more muted than among supporters of other candidates.

Estimation of Projection and Its Consequences

It is often argued that voters might engage in projection, such as in "wish fulfillment," believing that their preferred candidate is more likely to win than others do. Alternatively, there may be "bandwagon" effects, with some voters evaluating winning candidates highly only because they are winning. Bartels (1985, 1988) and Brady and Johnston (1987) provide estimates that indicate that voters in presidential primaries do make such projections. Here, we estimate a simple simultaneous equation model to test for the presence of such relationships between the candidate evaluation and viability measures and examine the consequences of these estimates for our findings.

If either sort of projection occurs, models that fail to take projection into account will yield biased (and inconsistent) estimates. In our case, the results would likely *overestimate* the extent of straightforward voting and *underestimate* the extent of sincere and sophisticated voting. More voters, that is, would report that their highest-rated candidate is the most viable candidate than would be the case with no projection. Thus, fewer would appear to be casting sophisticated or sincere votes. While projection, therefore, should lead our estimates to be conservative, it is important to estimate the extent of projection and its consequences.

We developed a simultaneous equation model with two equations, one for candidate evaluations and the other for viability ratings, and estimated the model by three-stage least squares. The feeling thermometer equation includes two variables in addition to viability assessments. One is a summary measure of the respondent's assessments of the candidates' traits, scored so that a positive coefficient is expected.[12] The second is the distance between the respondent's self-placement and the placement of the candidate on

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TABLE 8.

Multinomial Logit Estimates of Democratic Vote Preference, Given Candidate Evaluations and Assessments of Candidates' Viability

	GEPHARDT	GORE	JACKSON
Dukakis			
Evaluation	−.1405*	−.1479*	−.1275*
	(.0295)	(.0275)	(.0267)
Viability	−.0475*	−.0563*	−.0355
	(.0221)	(.0231)	(.0221)
Gephardt			
Evaluation	.0975*	.0364	−.0374**
	(.0251)	(.0221)	(.0209)
Viability	.0844*	.00310	.0272
	(.0230)	(.0172)	(.0172)
Gore			
Evaluation	.0738*	.1329	.0192
	(.0293)	(.0269)	(.0203)
Viability	−.0183	.0398*	.00632
	(.0167)	(.0186)	(.0159)
Jackson			
Evaluation	−.0521*	.00380	.0806*
	(.0177)	(.0157)	(.0188)
Viability	.0282	.00543	.0339*
	(.0174)	(.0149)	(.0143)
Constant	−1.3426	.0781	2.8613
	(1.9148)	(1.7651)	(1.7222)
−2xLLF (χ^2_{24})		270.33*	
Number of cases		192	

Note: Dukakis serves as baseline candidate. LLF denotes Log of Likelihood Function. Standard errors are in parentheses.
* $p \leq .025$ (one-tailed test), $\leq .05$ (two-tailed test).
** $.025 < p < .05$ (one-tailed test).

the seven-point ideology scale, with a negative coefficient expected. We also included a dummy variable for blacks' evaluations of Jackson.[13] These variables are assumed to have no effect on viability ratings.

The viability equation includes the thermometer score and a variable measuring the candidate's standing. This measure was based on actual poll results at the time of the interview and is, therefore, literally exogenous.[14] We also included combined candidate standing with our measure of the respondent's political involvement, on the hypothesis that the more involved are more likely to have more accurate assessments of candidate standing. Positive signs are expected for both of these variables.

We estimated these models for each party and for the three candidates included in our tests of the calculus of voting. The critical questions focus on the impact of the endogenous variables on each other. We also report a system R-squared for a measure of overall fit of the two-equation model. The correlation of error terms between the two equations (labeled the cross-r) provides a general indication of the potential for remaining endogenous relationships unaccounted for in the model.

The estimates of these models are reported in Table 9. The fit of these models is generally adequate, and the cross-equation error term correlations are gratifyingly low. Moreover, the signs of exogenous variables are generally correct and often significant (with the second and third Democratic candidate equations being the principal exceptions).

The critical question concerns the impact of the endogenous variables. The results are fairly clear. Viability assessments have a small—and often insignificant—impact on candidate evaluations, while those evaluations have a consistently large and significant impact on the viability measures. To be sure, viability is significant in the second and third Republican candidate equation; but the coefficient estimates are small (less than one-half the size of the smallest coefficient estimate for the thermometer evaluations). The one large coefficient (in the third Democratic equation) has a very large standard error, indicating unreliability. Moreover, the magnitude of the coefficients typically increases going from highest, to second-highest, to lowest-rated candidate—precisely the opposite of bandwagon reasoning.

Candidate evaluations, however, have large and significant effects on viability judgments. One might have expected, however, that "wish fulfillment" would have been concentrated in assessing the chances of the most highly rated candidate. The estimates for Republicans decline only slightly, moving from highest- to lowest-rated candidate; and they

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TABLE 9.

Estimates of Reciprocal Relationship between Candidate Evaluations and Viability Assessments, by Party

CANDIDATE RANKING	REPUBLICANS FIRST	REPUBLICANS SECOND	REPUBLICANS THIRD	DEMOCRATS FIRST	DEMOCRATS SECOND	DEMOCRATS THIRD
Evaluation equation						
Viability	−.073	.080*	.189*	.084	−.029	−.835
	(.065)	(.044)	(.071)	(.127)	(.228)	(.575)
Traits	14.990*	14.033*	15.871*	19.979*	15.284*	27.503*
	(1.41)	(1.27)	(1.70)	(2.09)	(2.65)	(8.38)
Ideology	−.985*	−1.473*	−2.406*	−0.485	−1.586*	−2.522
	(.518)	(.492)	(.606)	(.633)	(.696)	(1.58)
Jack. dum.	—	—	—	2.614	3.789	—
				(2.46)	(7.34)	
Constant	34.513*	19.123*	−5.789*	11.292	14.788*	10.132
	(5.11)	(4.56)	(4.54)	(6.66)	(6.20)	(10.5)
Viability equation						
Evaluation	.889*	.650*	.627*	.395*	.661*	.878*
	(.143)	(.115)	(.079)	(.127)	(.128)	(.113)
Standing	6.080*	8.244*	4.757*	−.206	.059	−.057
	(.814)	(.805)	(.854)	(.182)	(.181)	(.164)
Involv*stand	.018	−.055	.395*	.112*	.025	.071*
	(.165)	(.172)	(.185)	(.050)	(.049)	(.046)
Constant	−29.398*	−20.345*	−4.647	28.770*	8.833	9.415*
	(12.5)	(8.04)	(4.10)	(9.74)	(7.71)	(5.17)
Sys. R^2	.233	.350	.368	.177	.202	.151
Cross-r	−.246	−.363	−.495	.059	−.238	.304
	501	450	449	444	427	381

Note: See text for definition of variables. Standard errors are in parentheses.
* $p \leq .05$ (one-tailed test, constant two-tailed test).

move in exactly the opposite direction for Democrats. While this pattern is more reminiscent of response set bias than projection, it nonetheless indicates that we must examine choice in light of projection.

Since projection seems apparent only with viability measures, we computed a set of estimates of viability responses "purged" of the estimated projection of evaluations. These "purged" estimates were determined by computing the predicted viability ratings from the three-stage least squares estimates and subtracting off the projection (i.e., the thermometer score times its estimated coefficient). We used these to compute new ordinal classifications of voter types (comparable to Table 3) and reestimated the tests of the calculus of voting (comparable to Tables 5 and 6). In some respects, estimates of choice controlling for projection provide a test of a more dynamic model, in the sense that they attempt to capture the processes through which the voters reach their decisions.

With the Democratic estimates of wish fulfillment yielding coefficients that actually increased with decreasing ranking of the candidates, the proportion of straightforward voters increased slightly (from 43.8% to 48.3%), the proportion of sincere voters was halved (from 13.1% to 7.2%), and the proportion of sophisticated voters was exactly the same (13.1%). Among Republicans, the heavier discounting of the "purged" viability of the first-rated candidate, due to the large impact of evaluations on those viability assessments, led to a dramatic decrease in the proportion of straightforward voters (down from 52.8% to only 6.0%) and an equally dramatic increase in apparently sincere voting (up from 10.3% to 55.9%) and little change in apparently sophisticated voting (12.7% vs. an original 13.9%). Roughly speaking, the difference is that in the original measures, Bush was often the first-ranked candidate in thermometer and viability scores (i.e., the most preferred and most viable candidate). If we eliminate the effect of evaluations on viability, Bush (or whoever is rated most highly) is no longer the most viable candidate, so that voting for the most preferred candidate is now apparently sincere rather than straightforward.

It is perhaps more important to consider the impact of the "purged" viabilities on estimates of the calculus of voting, since the categorization does not take magnitudes into account and is not a direct test of sophisticated voting. Our results are presented in Table 10. Since the effect of projection is concentrated on the viabilities, we reestimated the models reported in Table 5. These include only probability terms and are therefore more affected by projection than the model estimates in Table 6.[15] It is true that the fit of the models and the estimate of each coefficient are

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TABLE 10.

"Purged" Measures of Candidate Viabilities

INDEPENDENT VARIABLES	MLE		
	REPUBLICANS	DEMOCRATS	BOTH PARTIES
P_{12}	1.139*	1.034*	.662*
	(.271)	(.231)	(.139)
P_{13}	.745*	.299	.214*
	(.236)	(.224)	(.130)
Constant	1.846*	.982*	1.308*
	(.170)	(.090)	(.0585)
−2xLLF (χ^2_2)	29.35*	44.25*	44.79*
Number of cases	503	483	986

Note: Replication of models reported in Table 5. P_{xy} denotes the difference in normalized viability scores given to candidates x and y. LLF denotes Log of Likelihood Function.
* $p \leq .05$ (one-tailed test of coefficient estimates).

smaller than in Table 5. Moreover, P_{13} in the Democratic case is much smaller and no longer statistically significant. Beyond that, however, there is remarkable similarity between these estimates and those based on the original measures. We therefore conclude that while there was a significant projection of evaluations onto viability assessments, corrections for projection seem to overcompensate (Bush *was* the front-runner in 1988). Even so, the corrections only attenuate, but do not reverse, any of our conclusions.

CONCLUSION

Citizens decide how to cast their presidential primary votes in sensible, even rational, ways. Over 80% of the respondents in each party had a vote preference consistent with the assumption of rationality, whether that was a straightforward, apparently sincere, apparently sophisticated, or rational-but-uninteresting type (see esp. Table 4).

In the post-1968 presidential nomination system, candidate fortunes have risen and fallen, sometimes with startling rapidity. Data from the 1988 NES Super Tuesday survey indicate that citizens responded to electoral events (here the Iowa caucuses and New Hampshire primary) by revising their perceptions of candidate viability in ways that corresponded to these events. While their evaluations of the candidates changed, these changes were far more muted. Vote preference changed dramatically, more in line with changing perceptions of viability than candidate evaluations. Moreover, the changes in voter types over the various campaign periods among the supporters of the various candidates further support this conclusion.

Our results further suggest that the changes in vote preferences were due to the rational weighing of the comparative chances of the candidates, along with the less variable (at least in the aggregate) comparative evaluations of the candidates. We tested the multicandidate calculus of voting in three ways. Each test led to the conclusion that both Republicans and Democrats had vote preferences consistent with sophisticated voting. Moreover, the direct tests of the calculus of voting (reported in Tables 5 and 6) showed that the specific parameter estimates were remarkably similar for both parties.

In our final models, in which the hypotheses of sophisticated versus sincere voting could be examined, we found that supporters of most candidates were estimated to have weighed both candidate evaluations and viability assessments. Robertson support was not significantly related to assessments of his chances; but it was significantly related to assessments of the chances of his major opponents. This does not fit neatly into purely sophisticated or purely sincere voting models, but it does provide evidence of strategic reasoning in line with the calculus of voting. Jackson supporters, by comparison, appeared to weigh both his chances and (although with less statistical certainty) those of the comparison candidate, Dukakis. As with Robertson, then, those with a vote preference for Jackson were consistent with the calculus of voting; but (also as with Robertson's supporters) the impact of viability assessments were more muted. Overall, then, most respondents followed the reasoning assumed in the multicandidate calculus of voting.

Respondents do seem to engage in projection, tending to rate more highly the chances of the candidate they like the best. But our estimates indicate that they also rate more highly the chances of their second- and third-ranked candidates. Even when projection effects are taken into account, the overall number of sophisticated voters was affected very little. Like voters in other democracies, U.S. citizens appear to avoid wasting their votes, when they are confronted with a multicandidate campaign in which the wasted vote logic applies.

We have demonstrated that citizens made sophisticated choices in only a single set of primaries—one

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that may have provided especially clear cues about the candidates' prospects, particularly among Republican contenders. Further research is clearly necessary both to specify the conditions under which sophisticated voting occurs and to understand better the dynamics of voters' choices up to the moment of their actual voting decisions.

Despite the need for additional research, our findings yield important insights about the democratic process. Amid a nomination process often referred to as chaotic and capricious, our evidence nonetheless suggests that primary voters are not simply fickle or that they jump on bandwagons because they like to support winners. Instead, our evidence suggests that overall, they make reasoned and sensible choices, seeking to support the office seeker who best combines feasibility as a candidate with attractiveness as a potential nominee.

Notes

The data for our analyses were made available by the Inter-University Consortium for Political and Social Research. This research was conducted in part while Aldrich was a Fellow at the Center for Advanced Study in the Behavioral Sciences. He would like to thank them and the National Science Foundation for its support, through grant no. BNS 87-00864. These institutions bear no responsibility for our analyses and interpretations. We would like to thank John Brehm for his counsel and John Geer for his comments.

1. Voters may also consider the candidates' chances of winning the general election. We investigated this possibility in Abramson et al. 1990. The evidence suggests that there is little difference between using measures of viability and using measures of electability.

2. In addition, 1,688 of these respondents were interviewed by telephone after 8 March. For a full description of this survey, see Center for Political Studies 1988.

3. Regardless of whether they planned to vote or not, respondents were asked which primary they would vote in if they did vote. Respondents who said they would vote in the Democratic primary were asked which Democrat they would vote for. Those who said they would vote in the Republican primary were asked which Republican they would vote for. Those who were not sure or did not know which primary they would vote in were asked how they would vote in both primaries. We call this measure *vote preference*, since it was asked even for those who did not intend to vote on 8 March.

4. We use the date of completion of the interview, which may differ from the date the survey was begun, to determine the time period in which an interview was conducted. In this and subsequent tables, we classify voters as Republicans or Democrats based on the primary in which they intended to vote or in which they said they would vote if they voted. We do not base this decision on candidate preference except for those unsure about which primary they would (or might) vote in.

5. We also estimated all of our statistical equations using the original thermometer scores in place of the normalized scores. There were no substantive differences in the estimates.

6. We present these results for all of the subcategories in Abramson et al. 1990, table 5. For example, among those classified as straightforward, we show the proportions who are classified as straightforward by evaluations and viability, by evaluations only, and by viability assessments only. For the distribution of subcategories for Table 4, see Abramson et al. 1990, table 9.

7. We include respondents in our analyses who reported that they did not intend to vote on 8 March and those who, when reinterviewed after that date, reported that they did not participate. There is very little difference between respondents who did, and those who did not, intend to vote and also very little difference between those who reported voting and those who reported not voting (Abramson et al. 1990, app. 2).

8. In addition to multicollinearity among the independent variables, the relatively small numbers of Republicans and of Democrats who preferred their second-ranked candidate limits the ability to absorb many degrees of freedom and produce meaningful estimates (further complicated by the asymptotic properties of probit and logit). Moreover, missing data loom large, especially for viability (and, in later models, utility) measures for minor candidates. See Abramson et al. 1990 for procedures and comparisons of models similar to those reported here and models containing larger numbers of comparisons.

9. Estimation of models that include P_{23} (and thus exclude either P_{12} or P_{13}) yield the hypothesized negative (and statistically significant) coefficient.

10. Since both of these models combine utility and viability measures, it could be argued that only one of them is driving the results. Perhaps, for example, all that matters is utility, not viability (i.e., respondents prefer to support candidates based only on utility considerations, or they back winners regardless of preferences). We tested models using just utility differentials and using just viability differentials. The results were dramatic and clear cut: goodness-of-fit statistics of the alternatives were far smaller than for the model reported in Table 6. In particular, the goodness-of-fit measures for normalized-utility-only models were 12.43 and 2.37 for Republicans and Democrats, respectively, while the comparable models for viability-assessments-only were 8.80 and 1.81, respectively. As these numbers suggest, the appropriate hypothesis tests are easily significant, indicating that for both parties the combined models are far superior to either of the individual models.

11. Too few respondents supported any other candidate except Hart to make analysis feasible. There was substantial support for Hart, but it collapsed after the Iowa caucuses and New Hampshire primary. While his collapsing support is consistent with the calculus of voting logic, including him would increase missing data substantially.

12. The candidate character variable is an average of responses to the eight measures of traits, such as "moral" and "decent," asked about each candidate in the Super Tuesday survey. The average was computed for all responses given, provided that at least six questions were answered. A factor analysis of these measures resulted in each candidate's traits loading on a unique dimension with similar magnitudes for each of the trait responses.

13. This appears only in the equations for the first- and second-highest-rated candidate, because no black included in the estimations rated Jackson third-highest. We considered including such a variable for white fundamentalists' evaluations of Robertson, but there were too few cases for reliable estimates.

14. Respondents were assigned to one of five time periods based on the date their interview was completed. The candidate standing variable for each candidate was an average of opinion poll results and any primary and caucus results in the relevant period. The periods and results used are

1. 17 January–2 February
   Des Moines Register Iowa poll (24 January)
   NYT/CBS national poll (26 January)
   Roper poll (South, 31 January)
2. 3–8 February
   Roper (South, 31 January)
   NYT/CBS national poll (2 February)
   Gallup poll (Iowa, 4 February)
3. 9–16 February
   Roper (South, 31 January)
   NYT/CBS national poll (2 February)
   Iowa caucuses

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4. 17–22 February
   Roper (South, 31 January)
   NYT/CBS national poll (22 February)
   Iowa caucuses
   New Hampshire primary
5. 23 February–8 March
   NYT/CBS national poll (22 February, and South 4 March)

These objective measures of standing were drawn from the *New York Times*, except for the Gallup Poll which was taken from the *Gallup Opinion Report*, and applied to all candidates except Gore. Gore's standing was based only on the national and southern polls, because he did not compete in the Iowa caucuses or the New Hampshire primary.

15. We also reestimated those models; and the effect of projection and its correction via "purging" are less in those models than in those reported in the text. These estimates are available on request.

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POLITICAL PARTIES AND ELECTORAL MOBILIZATION: POLITICAL STRUCTURE, SOCIAL STRUCTURE, AND THE PARTY CANVASS

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As agents of electoral mobilization, political parties occupy an important role in the social flow of political communication. We address several questions regarding party mobilization efforts. Whom do the parties seek to mobilize? What are the individual and aggregate characteristics and criteria that shape party mobilization efforts? What are the intended and unintended consequences of partisan mobilization, both for individual voters and for the electorate more generally? In answering these questions we make several arguments. First, party efforts at electoral mobilization inevitably depend upon a process of social diffusion and informal persuasion, so that the party canvass serves as a catalyst aimed at stimulating a cascading mobilization process. Second, party mobilization is best seen as being environmentally contingent upon institutional arrangements, locally defined strategic constraints, and partisan divisions within particular electorates. Finally, the efforts of party organizations generate a layer of political structure within the electorate that sometimes competes with social structure and often exists independently from it.

One of the major good works of political parties is to engage citizens in the political process. Parties play an important role in democratic politics when they mobilize the electorate to turn out and vote, thereby involving citizens in democratic governance. These efforts on the part of parties are not unbiased attempts aimed at encouraging diffuse system support: they carry an explicitly partisan message. But in the necessarily free market of democratic electoral competition such self-interest is a virtue because it serves to educate and inform the electorate in a politically meaningful manner. Modern treatments of U.S. political parties often focus upon parties as formal secondary organizations that publish platforms, endorse and support candidates, and provide officeholders with a framework for coalition formation (Mayhew 1986). At the same time, it is important to remember that as agents of electoral mobilization, parties occupy an important role in the social flow of political communication. Thus, inevitably, party politics and partisan efforts at electoral mobilization include a heavy dose of social influence. When a party worker knocks on a citizen's door, calls a citizen on the telephone, or affronts him with a yard sign, an effort is being made by one individual to provide information that will influence the behavior of another individual.

Moreover, the motivation for such contact resides in a set of implicit assumptions regarding how voters make their political choices. Successful efforts at voter mobilization do not enlist party supporters in single-file fashion. Even at the height of the so-called machine era in party politics, jobs, favors, and assistance were not distributed on a tit-for-tat basis (one job for one vote). Such an enterprise would have bankrupted even the wealthiest party organization. Rather, party efforts at electoral mobilization inevitably depend upon a process of social diffusion and informal persuasion. If a party worker can succeed in convincing Sally to vote Democratic, then Sally might be able to convince Bill, and so on. The investment of party resources becomes more potent to the extent that the votes of individuals are interdependent because the initial contact has cascading consequences in the collective deliberations of democracy.

We ask three questions regarding the role of party organizations as agents of mobilization in contemporary U.S. politics.

1. Whom do the parties seek to mobilize, and what are the individual and aggregate characteristics and criteria that shape party mobilization efforts?
2. What are the intended and unintended consequences of these mobilization criteria? Are the parties able to identify and contact appropriate portions of the electorate?
3. What impact do party mobilization efforts have upon citizens who are contacted? Does this impact reach beyond the point of contact to affect the electorate more generally?

Before turning to these questions, we address several antecedent issues related to the environmentally contingent nature of organized partisan efforts at electoral mobilization.

PARTIES AND MOBILIZATION

Any analysis of partisan mobilization in the context of U.S. politics must address a seeming discrepancy: at the same time that political scientists have developed a general skepticism regarding the mobilization potential of party organizations, party activists continue to invest heavily in the effort. Indeed, the reported incidence of party contacts among the electorate is higher now than it was in the early 1950s (Krassa

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1985), a period of time during which political parties are generally thought to have been more vibrant.

Skepticism regarding the mobilization potential of party organizations is rooted in several lines of argument. Many scholars and journalists agree that party organizations have weakened dramatically over time. From this perspective, it is difficult to see how party mobilization efforts could be important. Further, viewed from the perspective of national surveys, party contacts are seen as having generally consistent but modest effects on turnout, with little or no effect on the direction of the vote (Kramer 1970–71; Krassa 1988, 1989).

How can the discrepancy be understood? Why would party organizations continue to invest so heavily in undertakings that yield so modest a reward? First, it is important to remember that even modest effects in a statistical sense may be consequential to the outcome of an election, thereby making party efforts worthwhile. Second, the demise of party organizations, particularly at the local level (Gibson et al. 1983, 1985) may have been seriously overstated. Third, most analyses that point toward weak parties are narrowly focused to exclude candidate-centered organizations—perhaps the dominant form of partisan electoral organization in U.S. politics. If we include the mobilization efforts of candidate-centered organizations as party activity—a wholly appropriate practice in the organizationally fractured world of U.S. political parties (Sorauf and Beck 1987, chap. 1)—locally organized party efforts can be quite substantial. Finally (and perhaps most importantly), more recent efforts at understanding party mobilization are typically undertaken from the vantage point of the national sample survey. But is it appropriate to judge the impact of party mobilization efforts based simply on the votes of contacted respondents? Or should we look beyond the reports of individual voters to examine the environment of party mobilization as it affects both the strategy and the consequence of mobilization efforts?

THE ENVIRONMENT OF PARTY MOBILIZATION

The efforts of parties and their candidates to mobilize the electorate are located geographically and socially within the context of particular structural and institutional settings. Political party organizations in Indiana face a different set of opportunities and constraints than party organizations in California or Illinois (Jewell 1984; Jewell and Olson 1982). Some of these constraints are legal and institutional. The advent of the direct primary revolutionized the conduct of party politics; but the revolution occurred along very different lines in different settings, depending upon the particular shape of the reform. These reforms, coupled with later national reforms, produce a synergism with truly profound consequences that are still unfolding (Shafer 1983, 1988). Electoral reforms in many states were designed with the explicit purpose of creating an environment hostile toward organized parties and their efforts, effectively divorcing electoral participation from partisan involvement (Hawley 1973). In other states an environment was created that proved to be much more congenial to organized party politics, maintaining a crucial and visible role for party organizations and partisan attachments (Bledsoe and Welch 1987).

It is not only the various legal and institutional settings of U.S. electoral politics that create the unique environments of party mobilization. As Beck (1974) and others have shown, party mobilization efforts must also be seen within the existing partisan balance of particular locales (also see Hofstetter 1973; Miller 1956; Putnam 1966). It is a common observation that U.S. electoral politics has become nationalized with a corresponding breakdown of regional electoral patterns; but the case for nationalization is too easily overstated, at least as it relates to one-party dominance in state and local politics. In their 1974–80 analysis of party competition within the states, Bibby and his colleagues (1983) classify only 22 states as being two-party-competitive. Moreover, one-party dominance within particular locales is even more widespread. The increased willingness of the U.S. voter to engage in split-ticket voting has meant that local one-party politics can survive even when one-party control runs in a direction opposite that of state and national trends. Indeed, Gibson and his colleagues (1985) show that the organizational vitality of state political party organizations is independent of the organizational vitality of local political party organizations. Hence, it is commonplace for U.S. voters to live, for example, in Democratic cities that lie within Republican states. Configurations such as these will be shown to have important political consequences.

Both environmental features loom especially large with respect to the present analysis: (1) the institutional environment of party activity, particularly as it affects the availability of information that parties might use in their mobilization efforts; and (2) the partisan balance that exists within a particular locale. Both features are part of the inescapable context within which party efforts are formulated, and both have substantial consequences for the strategies of party organizations.

THE RESEARCH SITE: SOUTH BEND, INDIANA

We address the strategies and consequences of partisan mobilization through the analysis of a 1984 election study conducted in the South Bend, Indiana metropolitan area. South Bend proves to be an advantageous study site for several reasons. First, the city possesses a political environment that is relatively congenial to political organization and the conduct of partisan politics (Stabrowski 1984). While

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the state of Indiana does not possess a closed primary system, voters go on record as party supporters when they choose a party's primary ballot, and party organizations are able to identify the voters who participate in their primaries.[1] Second, the Democratic party has traditionally possessed an advantage in South Bend politics. Thus, we are able to examine the organizational and institutional implications of majority and minority standing. Third, the study of partisan mobilization is hampered by an exclusive focus on presidential politics; but important gubernatorial and congressional campaigns were also being waged in South Bend during the fall of 1984, and we will consider partisan mobilization with respect to these campaigns as well.

The argument we develop does not depend upon the selection of South Bend as a study site, and a skewed partisan environment does not make South Bend atypical of other U.S. cities. The short answer to those remaining skeptical is that everyone lives in a South Bend. The compelling reality of urban and suburban politics today is that almost all of us live and work as part of political majorities or minorities because few environments maintain a genuine political balance. Our neighborhoods are tilted one way, our workplaces another, and our cities perhaps another. The aggregation of micro environments to the typical levels of party organization is unlikely to create politically competitive units. Indeed, the continuing efforts of political elites are aimed at avoiding just such competition. But just because the partisan bias of an environment is highly skewed, it does not follow that party organization and political mobilization become irrelevant. A major proposition advanced here is that important and systematic political consequences flow from these normal and typically skewed partisan distributions.

The South Bend study included a three-wave panel survey of approximately 15 hundred respondents, where the second and third waves were supplemented with new respondents to offset panel attrition. All interviews were conducted through the use of computer-assisted telephone interviewing at the Center for Survey Research of Indiana University in Bloomington. Our analysis relies most heavily on information taken in the third, postelection wave. Two features of the South Bend study deserve special attention for purposes of this analysis. First, the 15 hundred respondents were chosen to be equally distributed across 16 neighborhoods. Respondents were randomly selected within the neighborhoods, but the neighborhoods were chosen purposefully to maximize social status variation across the neighborhoods and to minimize variation within the neighborhoods. All the neighborhoods were predominantly white in racial composition. We thus have random samples of more than 90 respondents in each neighborhood, which means that the analysis can operate at two levels. Any question included in the survey can be used as an individual measure, or it can be aggregated to provide a neighborhood measure.

Second, the survey information is augmented by public record data regarding voter registration and primary participation for individual citizens. This information is used to determine (1) whether respondents are registered to vote, (2) whether they voted in past elections, and (3) which party's primary ballot they requested when voting in a primary election. Thus, we can consider the citizens' self-reported behavior relative to the official record of the behavior (Katosh and Traugott 1981; Traugott and Katosh 1979), and all this within a particular political and social setting.

In summary, we aim at the analysis of a particular party system, particular party organizations, and a particular institutional setting. What relevance does such an analysis have for U.S. politics more generally? A major weakness of modern research on parties and party systems is that it is typically and implicitly predicated on the unsustainable myth that the relationship between parties and voters can be understood *apart* from a particular setting. Our argument lies in an exactly opposite direction: the relationship between parties and electorates is environmentally contingent and must be understood in that light. Only by appreciating the relationship between parties and voters in South Bend (and Los Angeles and Schenectady and Walla Walla) can we begin the task of making meaningful statements in more general terms (Boudon 1986).

THE STRATEGIC BASIS OF THE PARTY CANVASS

Perhaps the most important form of encounter between parties and citizens (and certainly the most widely studied) is the party canvass—the effort of party workers to contact individual citizens. About one-third of our South Bend respondents report being contacted by at least one of the parties, with 22% reporting contact by each of the parties and 10% reporting contact by both of the parties.[2] If party organizations are successful at targeting their potential supporters for contact, we should expect to see a strong negative relationship between the likelihood of being contacted by the Democrats and by the Republicans. But the South Bend data demonstrate a level of mutual contacting (.10) that is double the proportion that would be expected by chance alone ($.22 \times .22 = .046$), and this raises important issues: Whom *do* each of the parties contact? What criteria *do* the parties use in deciding whom to contact? Are the parties ignorant, or merely naive?

The official list of registered voters within a particular governmental jurisdiction is the most readily available source of information that most parties can use in their efforts to mobilize voters. State-to-state variations in voter registration arrangements are enormous, but in many states the record-keeping operations of the registrar provide a valuable source of information for political party efforts. Registration records identify the citizenry which is currently eligi-

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TABLE 1

Ballot Chosen in the Primary Election by Self-Reported Partisanship, 1982–84 (%)

	DEMOCRAT		INDEPENDENT			REPUBLICAN	
PRIMARY	STRONG	WEAK	DEM.	NO PARTY	REP.	WEAK	STRONG
1984							
Democratic	57.6	45.0	46.9	16.2	20.9	10.7	5.6
Republican	.0	.9	1.1	2.9	9.3	16.7	27.0
No vote	42.4	54.1	52.0	81.0	69.8	72.7	67.4
Number of cases	335	220	179	105	215	150	267
1983							
Democratic	53.4	40.4	33.5	20.0	19.1	10.0	7.9
Republican	.0	1.8	1.1	1.9	12.1	12.7	31.1
No vote	46.6	57.7	65.4	78.1	68.8	77.3	61.0
Number of cases	335	220	179	105	215	150	267
1982							
Democratic	40.6	32.7	33.0	12.4	13.0	6.0	3.8
Republican	.3	.4	.6	4.8	9.3	13.3	25.8
No vote	59.1	66.8	66.5	82.9	77.7	80.7	70.4
Number of cases	335	220	179	105	215	150	267

ble to participate in an election. In many states, registration records provide indirect evidence regarding the partisan loyalties of citizens. In some states voters must register with a particular party in order to participate in that party's primary. In other states, such as Indiana, voters do not register with a party, but go on record as party supporters when they request that party's ballot at a primary election.

How useful is this information as a guide to the predicted political behavior of a voter? Participation in St. Joseph County primaries is a less than perfect indicator of partisan loyalty (see also Finkel and Scarrow 1985). More significantly for the conduct of electoral politics, the choice of a party's ballot is a *biased* measure of partisanship because an asymmetrical relationship exists between partisanship and primary ballot choice. The dominance of the Democratic party in local politics creates a situation in which the winner of the Democratic primary is frequently the winner of the general election as well, and thus the stakes are often higher in the Democratic primary (Farquharson 1969; Key 1949). Table 1 combines (1) post-election survey data regarding partisan loyalty with (2) public record data regarding primary participation in the three previous primary elections, and thus we can compare partisan behavioral records across self-reported subjective loyalties. Table 1 shows that many Republican loyalists vote in Democratic primaries, but Democratic loyalists seldom vote in Republican primaries. While this sort of cross-party primary voting is substantial, Table 1 also shows that it has not reached epidemic proportions; thus, knowing which ballot a voter takes at a particular election still provides a useful indicator of partisan preference.[3] At the same time, ballot choice is a biased indicator; and the bias has important consequences for political organization and partisan mobilization.

CRITERIA FOR PARTY CONTACT

To what extent does information regarding primary participation affect the likelihood that a voter will be contacted by a party? We employ public record data regarding cumulative primary participation histories to address this question among our respondents. These cumulative histories provide information on as many as eight previous primaries for individuals who are registered to vote, and this information is used to determine frequency of participation and ballot choice among our respondents.

Among respondents who have never voted in a primary, approximately 25% report being contacted during the campaign. In contrast, the rate of reported contact increases to about 40% among other respondents. Not surprisingly, party organizations focus their contacting efforts on their own primary voters. Of those who only vote in Democratic primaries 32% report being contacted on behalf of Democratic candidates, and only 19% report a Republican contact. Among Republican primary voters, 34% report a Republican contact, and only 15% report a Democratic contact. Significantly, the Democrats demonstrate a rate of contacting citizens who have voted in both parties' primaries (23%) nearly equal to the Republicans rate (29%), even though Table 1 shows that such voters are almost entirely Republican in their professed political sympathies. It would appear that the Democrats are either misperceiving, miscalculating, or unable to repair a situation in which they contact voters who are likely to be Republican loyalists.

What other information might the party organizations use in their contacting efforts? South Bend politics is structured by local geography in some very pronounced ways. For example, the proportion of our survey respondents voting for Reagan within the

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16 sampled neighborhoods varies from .18 to .59; and the proportion voting for Mondale varies from .14 to .38.[4] Political strategists within the city are certainly aware of these patterns; thus, it is reasonable to expect that they use zone of residence as an indicator of likely political sympathies. Such a measurement device is especially useful because it allows the party to organize its efforts on a geographic (door-to-door) basis, rather than on the basis of a list of individuals. But how should parties decide *which* neighborhoods might provide a fruitful focus for their activity? Several criteria might be used: (1) they might select neighborhoods where the turnout for the party's primary was heaviest; (2) they might select neighborhoods that are high (or low) in terms of social status; (3) they might have complex, historically idiosyncratic reasons for selecting particular neighborhoods; and so on.

We have experimented with a variety of different aggregate measures that might potentially explain the individual incidence of party contact within our sample. The results of these efforts are shown in the top of Table 2, which displays logit models for the probabilities that a respondent is contacted by each of the parties. Party contact by a particular party is the criterion variable. The explanatory variables are cumulative participation records in Democratic and Republican primaries, and the neighborhood property that was found to be most influential in explaining each party's contacting efforts. The magnitudes of effects predicted by these models are shown in Figure 1. As this figure shows, the probability of Republican contact among our respondents is enhanced both by Republican primary participation and by living in a neighborhood with a higher turnout in the Republican party primary. Several neighborhood properties provided nearly equivalent statistical purchase in explaining Republican contact—the closest competitor being the mean level of educational attainment within the neighborhood. In other words, citizens are more likely to be contacted by the Republican party if they participate in Republican primaries. Independent of their own participation, they are also more likely to be contacted if they live among others who vote in Republican primaries; and this tends to occur in higher-status neighborhoods.

The Democrats exhibit a different pattern of contacting, at least with respect to aggregate neighborhood effects. Figure 1 shows that those who participate in Democratic party primaries are more likely to be contacted by the Democrats; but the only neighborhood property that helps to explain Democratic contact is simply the proportion of neighborhood respondents who report being contacted. Thus, people who live in neighborhoods with high levels of Democratic contact are more likely to be contacted, even after the individual criteria for contacting are taken into account.

Is it tautological to say that people living in high-contact areas are more likely to be contacted? We have removed the respondent's own experience when calculating the proportion of neighborhood respondents who were contacted by the Democrats; thus, the relationship is not tautological. For example, if a respondent lives in a neighborhood where 20% of the people were contacted by the Democrats, this means that 20% of all *other* respondents living in the neighborhood were contacted. Indeed, the individual probability of being contacted by the Republicans is *not* significantly affected by the level of neighborhood contacting after the individual contacting criteria are taken into account.

In one sense, this account of Democratic contacting is not entirely satisfactory, because it does not provide a rationale for the higher incidence of contacting in some neighborhoods. For some reason (perhaps related to traditional areas of Democratic strength; see Stabrowski 1984), the Democratic party tends to concentrate its resources within particular neighborhoods. At the same time, our inability to explain this concentration of effort is revealing in another respect: it shows that *political structure*, as well as social structure, is a significant element in political affairs. Political outcomes cannot be explained wholly in terms of class and status; they must also be explained in terms of party. Indeed, in the sense we are defining it here, political structure provides a separate level of meaning and experience that is imposed upon social structure (Key and Munger 1959).

THE EFFECTIVENESS OF THE PARTY CONTACTING CRITERIA

So far, we have addressed the factors that serve as criteria for party contact—information that the parties use to locate and mobilize their supporters. This information provides only an indirect measure of partisan orientation; but it is the best that the parties can do, because it is, in essence, all the information that is available. In other words, the party is not able to interview all the voters prior to contacting them! If they *were* able to conduct such interviews, they might obtain a more direct measure of partisan orientation that left less room for doubt regarding political intentions.

While such information is unavailable for the entire electorate, it *is* available for the respondents who participated in the survey. Thus, we can assess the success of each party in locating its supporters. The bottom of Table 2 replicates the analysis of the top but includes individual-level controls for self-reported partisanship (party identification) and individual educational attainment, allowing us to address the question, To what extent do the contacting criteria serve as valid indicators of party support? If the introduction of a control for each respondent's partisanship diminishes the explanatory power of the contacting criteria, then the contacting criteria can be assumed to be doing a good job for the party.

The introduction of these controls produces very different consequences for the analysis of Democratic

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TABLE 2

Republican and Democratic Contact by Primary Election Participation and Selected Neighborhood Characteristics, with and without Controls for Party Identification and Education.

EXPLANATORY VARIABLES	CONTACT: DEMOCRATIC	CONTACT: REPUBLICAN
How do the parties decide whom to contact?		
Constant	−2.25 (8.93)**	−1.70 (10.99)**
Cumul. particip. in Dem. primaries[a]	.17 (7.76)**	−.04 (1.42)
Cumul. particip. in Rep. primaries[a]	−.03 (.74)	.16 (4.30)**
Nghbd. proport. contacted by Democrats[b]	2.73 (2.69)**	—
Nghbd. proport. voting in 1984 Rep. primary	—	4.09 (2.86)**
Whom do the parties contact?		
Constant	−2.70 (5.96)**	−3.24 (8.29)**
Cumul. participt. in Dem. primaries	.15 (6.46)**	.01 (.45)
Cumul. participt. in Rep. primaries[a]	−.01 (.23)	.08 (1.96)*
Nghbd. proport. contacted by Democrats[b]	2.77 (2.72)**	—
Nghbd. proport. voting in 1984 Rep. primary	—	1.94 (1.29)
Indiv. educ.[c]	.05 (1.72)	.09 (3.09)**
Indiv. party ID	−.06 (1.56)	.16 (4.39)**

Note: Logit model; coefficient t-values in parentheses. N = 1,446 for all models.
[a]Range 0–8.
[b]The respondent's own contacting experience is excluded in the calculation of this neighborhood proportion for that respondent.
[c]Years of school.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

and Republican contacting efforts. In terms of Democratic contact, neither individual education nor party identification produce particularly strong, statistically discernible effects. Figure 2 shows that the originally estimated effects of Democratic primary participation and Democratic contacting in the neighborhood are essentially maintained. In contrast, for Republican contact, both individual education and party identification produce strong, statistically discernible effects; and the originally estimated effects are substantially reduced in magnitude (see Figure 2).

What does this suggest? It would seem that the Republicans are doing a better job than the Democrats in contacting their supporters. When we hold primary participation constant, the probability of being contacted by the Democrats is, at most, only modestly affected by class and party. Under the same conditions, the probability of being contacted by the Republicans is dramatically affected by class and party. Why is this the case? Part of the answer lies in Table 1. When the Republicans contact a participant in Republican primaries, they can rest assured that they have contacted a voter with Republican sympathies. Even when the Republicans contact a voter who is a switch-hitter in primary voting, they can still be relatively confident that they have made connection with a Republican sympathizer. In contrast, even when the Democrats contact a voter who participates exclusively in Democratic primaries, they still cannot be confident that they have located a voter with Democratic loyalties.

Ironically, this set of circumstances is generated by the *strength* of the Democratic party in the South Bend area. Democratic dominance in local politics has produced a situation in which Republican primaries frequently become only a sideshow in local political affairs (Key 1949). As a result, many Republicans regularly participate in the affairs of the Democratic

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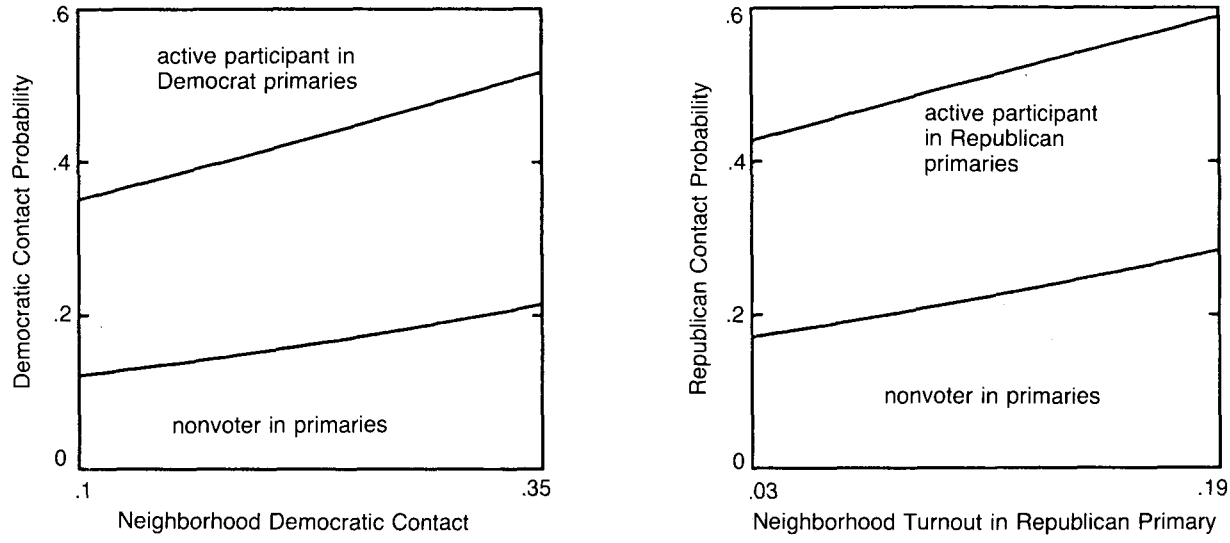
Figure 1

How do the parties decide whom to contact?



Note: Table 2 estimates without controls for education or partisanship. An active participant is defined as someone who has voted in eight primaries.

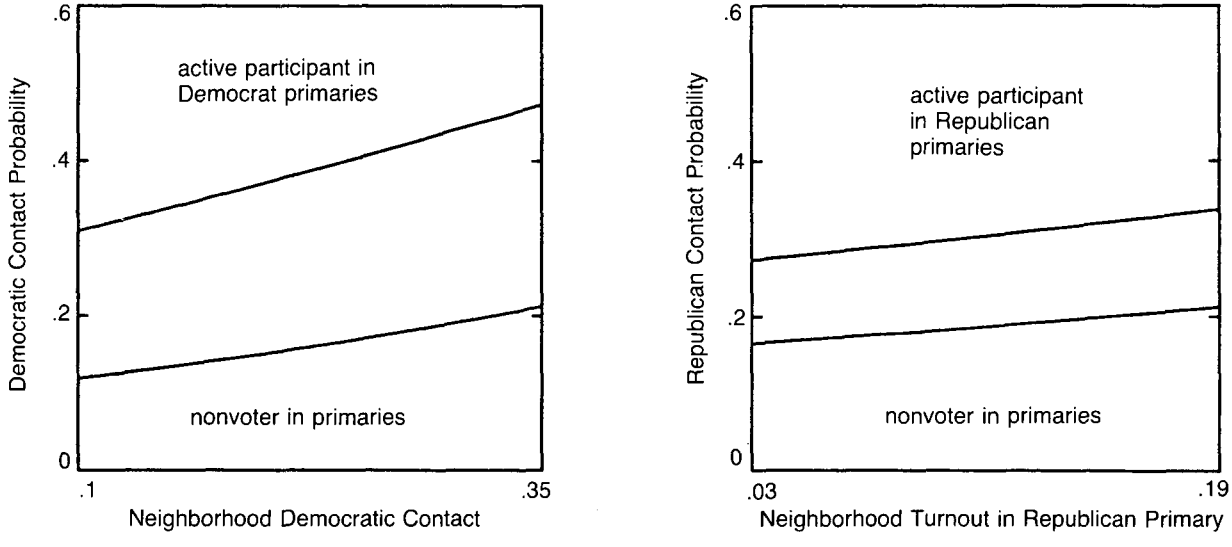
primary. But this produces a situation in which it is often difficult to tell the players apart. It becomes difficult, especially for the Democrats, to identify their supporters. Thus, the majority party is placed at an institutional disadvantage (see Katz and Eldersveld 1961).[5]

THE AGGREGATE STRUCTURE OF THE PARTY CANVASS

If we adopt the premise that citizens' political choices are socially interdependent—that the behavior of one

Figure 2

Whom do the Parties Contact?



Note: Table 2 estimates with controls for education and partisanship. Education is controlled at 12 years and party identification is controlled at *independent*. An active participant is someone who has voted in eight primaries.

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individual is affected by the behavior of other individuals who share the same social space—then the aggregate pattern of party contact becomes at least as important as the micro pattern. When the Republicans contact a voter who lives in a particular neighborhood, works at a particular office, and bowls in a particular league, then the party has potentially gained access to the neighborhood, the office, and the bowling alley. The ripple effect of social interaction produces consequences that lie beyond the initial contact and extend more generally to the social space that is inhabited by the contacted individual (Eulau 1986; Huckfeldt 1986; Huckfeldt and Sprague 1987).

We have already seen that parties and candidates impose a political structure upon the electorate that is distinct from important social structural elements. We cannot easily predict the individual likelihood of contact simply with reference to aggregate social structural properties within the individual's environment. Much of this is undoubtedly due to the individuation of party contacting strategies (to the use of voter registration lists); but it does not follow that individualized strategies are lacking in aggregate consequences, even if the consequences are unforseen. Rather, a new layer of aggregate organization is imposed upon an already-complex set of well-organized aggregate properties. Perhaps the best evidence for this politically imposed structure is the range of variation in the level of contacting between the neighborhoods included within our sample. The proportion of neighborhood respondents who report being contacted varies from .12 to .36 and from .11 to .34 for the Republicans and Democrats, respectively. A natural question thus arises: How does the aggregate structure of party mobilization relate to other structural characteristics within the community?

Table 3 displays the correlations between neighborhood patterns of party contacting and a number of other, neighborhood-level social and political characteristics. This table provides an aggregate picture of the political and social structure of party mobilization efforts during the election campaign. The neighborhood characteristics are measured by aggregating survey responses within each of the 16 study neighborhoods, thereby providing a point estimate within each neighborhood based on an average sample of approximately 90 respondents.

These correlations are revealing on several counts. First, the aggregate patterns of Democratic and Republican contacting efforts are only weakly related to each other. In the context of individual contacting, we have already seen that the probabilities of being contacted by the Republicans and Democrats are slightly correlated in a positive direction. Now, in the case of aggregate patterns of contacting, we see a pattern of near independence with a slightly negative relationship between Democratic and Republican efforts. In both instances, however, the relative independence of mobilization efforts is more striking than the presence of any weak relationship. Once again, if both parties were efficiently contacting their potential supporters, we might expect to see a strong negative relationship between the mobilization efforts.

TABLE 3

Correlations between Neighborhood Patterns of Party Contacting and Various Political and Social Characteristics

EXPLANATORY VARIABLES	PROPORTION CONTACTED BY DEMOCRATS	PROPORTION CONTACTED BY REPUBLICANS
Proport. contacted by Democrats	1.000**	−.080
Proport. contacted by Republicans	−.080	1.000**
Mean educ. (school years)	−.220	.564*
Mean party ID	−.378	.685**
Proport. voting in Dem. primary	.274	−.364
Proport. voting in Rep. primary	−.106	.631**

Note: The N for each correlation is 16 neighborhoods. Neighborhood characteristics are measured on the basis of 1,446 interviews sorted by 16 neighborhoods.
*$p < .05$, two-tailed test.
**$p < .01$, two-tailed test.

Much of the independence between party mobilization efforts is due to the very ambiguous structure that underlies Democratic mobilization. Using aggregate party identification as an invisible criterion of contacting success (invisible, at least, to party strategists), the neighborhood-level relationship between mean party identification and proportion contacted is much stronger for the Republicans: .68 compared with −.38. This means that 46% of the aggregate pattern of Republican mobilization effort is explained by neighborhood partisanship, while only 14% of Democratic mobilization effort is similarly explained. The Republicans are more than three times as efficient as the Democrats at this crucial discriminatory task. If the goal of mobilization is to activate the party's friends, then the Republicans are much more effective at this effort.

Finally, Table 3 also provides information regarding the class basis of party mobilization efforts. The aggregate structure of party contacts is more closely related to the aggregate structure of educational attainment (our measure of social class) for the Republicans than for the Democrats: .56 compared to −.22. In words, Republican mobilization efforts are more strongly oriented toward upper-status locales than Democratic efforts toward lower-status locales. In terms of explained variation, this presents a sixfold difference (.05 compared to .31) in the ability of class to account for aggregate variations in party mobilization effort! Why is this the case?

Even though the Democrats' natural constituency is located among lower-status citizens, the *class structure* of mobilization runs at least partially counter to the *political structure* of mobilization. When the Dem-

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ocrats focus their mobilization efforts on citizens who have indicated Democratic sympathies by voting in Democratic primaries, they are introducing a mobilization criterion with an upper-status bias because primary participants are more likely to be upper-status citizens (see Krassa 1985). This is seen in the aggregate correlations between education and participation in each of the party primaries. The correlations lie in the expected opposite directions, but the relationship between education and Republican primary participation is considerably stronger.

In summary, this examination of the aggregate structure of party mobilization points once again to the dilemma of the majority party. The Democrats are unable to impose a cohesive structure on their efforts for two separable reasons. First, Democratic primary participation is an especially ambiguous criterion of party support. Many Democratic primary voters are not loyal Democrats; thus, the party has a difficult time identifying its supporters. In this light, it is not surprising that the pattern of Democratic contacting is less closely linked to the pattern of Democratic primary voting. Second, the political criterion for identifying supporters tends to run in an opposite direction from the class basis of party support. Primary participation is more widespread among upper-status citizens, and contacting primary participants thus tends to dilute the class basis of Democratic organization. In other words, social structure tends to lose out to political structure. Particularly for the Democrats, it thus becomes clear that the unintended consequence of party mobilization activity is an aggregate structure of contacting that is only weakly related to the social structure underlying party support.

TABLE 4

Democratic and Republican Signs and Stickers As a Function of Neighborhood Partisanship, Party Contacts, and Opposite Party's Signs and Stickers

EXPLANATORY VARIABLES	YARD SIGNS AND BUMPER STICKERS	
	DEMOCRATIC	REPUBLICAN
Constant	.10	−.14
	(4.29)**	(2.83)*
Nghbd. partisanship[a]	−.021	.02
	(4.23)**	(1.30)
Nghbd. proport. contacted by party	−.02	.32
	(.26)	(2.36)*
Oppos. party's signs and stickers[b]	.27	1.39
	(2.88)*	(3.25)**
R^2	.65	.65
Number of cases	16	16

Note: Least squares models. Coefficient t-values are in parentheses.
[a]Mean party identification.
[b]Proportion.
*$p \leq .5$, two-tailed test.
**$p \leq .01$, two-tailed test.

AGGREGATES AND INDIVIDUALS

To reiterate, the aggregate distribution of these behaviors is just as important as the individual distributions; that is, it is just as important to recognize the aggregate consequences of individual behavior as it is to recognize the individual consequences of aggregate behavior (Sprague 1976). Party strategists adopt a set of strategies that determine the individual likelihood of contact. As we have seen, however, these individualized strategies do not translate unambiguously into aggregate consequences.

This imperfect match between politics at the individual level and politics in the aggregate extends to other forms of electoral involvement, as well. Consider a very visible manifestation of individual involvement with important aggregate manifestations—the placement of bumper stickers and yard signs. Such behavior is important in the aggregate because it sends a signal regarding appropriate behavior to others who are exposed to these signs. Moreover, exposure is selective. The people who are most likely to see the yard signs and bumper stickers are those who share the same living space. Thus, a confluence of individual interest and socially transmitted influence is produced. People who live around an individual (people who are like the individual and have many interests in common) send a very visible aggregate message regarding appropriate political behavior. As a result, the individual receives a cumulative signal regarding the political preferences of those whom he or she is likely to perceive as sharing the same interests and viewpoints (Burt 1987; Downs 1957; Huckfeldt and Sprague 1991).

How should we expect the *distribution* of signs and stickers to vary across neighborhoods? A first response might be that Democratic yard signs should be more prevalent in Democratic neighborhoods, and Republican yard signs in Republican neighborhoods; but Table 4 suggests that this is only part of the picture. Neighborhood partisanship appears to be important for the distribution of Democratic signs and stickers, but not for Republican. Republican variation is better explained by the aggregate pattern of Republican contacting.[6]

Finally, Table 4 also suggests a symbiotic effect: the presence of Democratic signs and stickers tends to stimulate Republican signs and stickers, and the presence of Republican signs and stickers tends to stimulate Democratic signs and stickers. Why might this cross-stimulation occur? Some individuals are, undoubtedly, goaded into action by visible signs of support for a candidate they oppose. Just as likely, the distribution of yard signs in particular may be strategically controlled. The placement of signs may be calculated as a response to a higher density of signs in a particular area of town. Along these same lines, it is interesting to note the stronger effects on the distribution of Republican signs and stickers that

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TABLE 5

Neighborhood Ratio of Democratic Signs and Stickers to Republican Signs and Stickers by Republican and Democratic Contact in the Neighborhood

EXPLANATORY VARIABLES	COEFFICIENTS
Constant	1.46 (2.52)*
Nghbd. proportion contacted by Democrats	8.03 (4.55)**
Nghbd. proportion contacted by Republicans	−9.05 (5.23)**
R^2	.80
Number of cases	16

* $p \leq .05$, two-tailed test.
** $p \leq .01$, two-tailed test.
Note: Least squares models. Coefficient t-values in parentheses.

occur as the result of both party contact and the opposite party's signs and stickers. If Republicans perceive themselves as a minority in South Bend—and particularly if they perceive themselves as a minority within a neighborhood—then they may behave more strategically than Democrats, as the rate of primary crossover voting already indicates (Farquharson 1969). In particular, confronted by excess Democratic yard signs and bumper stickers, they may well contact their known supporters to put up yard signs in response. In short, we may be seeing further evidence of the value that accrues to the political minority as a result of intense party activity (Katz and Eldersveld 1961).

The distribution of yard signs and bumper stickers reflects the complex interplay of partisan support, party strategy, and party competition. While we do not possess the precise menu that yields the aggregate result, we are able to identify many of the crucial ingredients. They are displayed in Table 4, where the ratio of Democratic to Republican signs and stickers is regressed on the levels of Democratic and Republican contacting in the neighborhoods.

Quite clearly, party activity makes a decided difference: 80% of the variation in the ratio can be explained in terms of party effort.

CONSEQUENCES OF PARTY EFFORTS

What are the consequences of party mobilization efforts for the citizens who are contacted? Do organized partisan efforts have results which extend beyond the point of contact? These are crucial questions that speak directly to the role of political parties in democratic politics and to the potential for a politically organized electorate. We address them here in the context of extensive previous work showing (1) generally moderate but consistent effects upon levels of turnout and (2) mixed evidence regarding effects on the direction of the vote (Crotty 1971; Cutright 1963; Cutright and Rossi 1958; Gosnell 1927; Katz and Eldersveld 1961; Kramer 1970–71; Krassa 1988, 1989; Pimlott 1972, 1973; Wolfinger 1963).

As we have seen, South Bend party organizations tend to pursue contacts among citizens who have voted in previous party primaries and thus have a record of party support. This is an important fact to take into account when judging the individual consequences of party contact. South Bend primary voters participated in the subsequent general election at an extraordinarily high rate. More than 98% of respondents who voted in the primary also voted in the general election. In contrast, nonparticipants in the primary were as likely to vote as to stay home at the general election. Thus, if an analysis ignores the high level of participation among primary voters, the importance of party contacting might easily be exaggerated because party contacting is keyed to primary participation.

In keeping with the importance of primary participation, the consequences of party contact are analyzed separately among primary election voters and nonvoters. The effects of party contacts upon turnout, presidential voting, gubernatorial voting, and congressional voting are considered among primary election nonvoters in the top of Table 6. In column 1, nonvoting is the reference condition, and the coefficients predict the probability of voting in the general election. In the remaining columns, voting for the Democratic candidate is the reference condition, and the coefficients predict the probability of voting for the Republican candidate. Thus, we can use the first model to consider the effect of contact upon turnout. And for those who *do* vote in the general election, we can employ the other models to consider the effect of contact on the two-party division of the vote. The top of Table 6 shows, at most, weak effects due to party contacting among nonvoters in the primary election. Both Republican and Democratic contact fail to generate statistically discernible effects at even the 95% level of confidence for any of the voting behaviors, although the respective coefficients all lie in the expected directions. In general, the more impressive effects in these four models are due to self-identified partisanship[7] and, in the case of turnout, to individual and contextual education.

The estimated effects of individual and contextual education on turnout (based on Table 6, column 1), with all other variables held constant, are shown in Figure 3. Respondents with higher levels of schooling are more likely to vote, regardless of the educational level of the surrounding neighborhood population. Conversely, respondents who live among a more highly educated population are more likely vote, regardless of their own educational levels (Tingsten 1963). Recall once again that the top of Table 6 is estimated on the basis of respondents who did not vote in the previous spring's primary election. Political structure has little effect on general election turn-

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TABLE 6

Voting Choices in the 1984 General Election

EXPLANATORY VARIABLES	TURNOUT	REPUBLICAN PRESIDENT	REPUBLICAN GOVERNOR	REPUBLICAN REPRESENTATIVE
Primary nonvoters				
Constant	−3.16**	−.51	−4.91*	−3.87
Dem. Identifier[a]	−.49**	−2.77**	−1.84**	−2.17**
Rep. identifier[a]	.26	2.10**	1.96**	1.45**
Age	−.02**	.02	.01	.02
Educ.[b]	.09*	.01	.02	−.06
Nghbd. educ.[c]	.24**	.08	.32	.31*
Dem. contact in nghbd.[d]	−.78	−.61	1.69	−.76
Rep. contact in nghbd.[d]	−.89	.36	−.03	.40
Dem. contact[a]	.35	−.73	−.12	−.44
Rep. contact[a]	.23	.19	.27	.33
Number of cases	853	373	374	374
Voters in the primary				
Constant	—	.70	−2.49	−.42
Dem. identifier[a]	—	−1.35**	−2.14**	−1.69**
Rep. identifier[a]	—	4.13**	1.70**	2.23**
Age	—	−.01	.00	.00
Educ.[b]	—	.04	.03	.06
Nghbd. educ.[c]	—	−.06	.14	−.14
Dem. contact in nghbd.[d]	—	.44	2.64	.59
Rep. contact in nghbd.[d]	—	.33	−.60	2.87
Dem. contact[a]	—	−.49	−.85**	−.83**
Rep. contact[a]	—	.46	.62*	.10
Number of cases	—	539	557	552

Note: Logit models.
[a]Dummy coded.
[b]Years of school.
[c]Mean school years.
[d]Proportion contacted.
*$p < .05$, two-tailed test.
**$p < .01$, two-tailed test.

out among these individuals lying outside the boundaries of conventional party involvement. In contrast, social structure has a decided effect upon whether they vote. For the other respondents (those who *did* vote in the primary election), the level of participation in the general election is so high that *both* social structure *and* party contact are rendered irrelevant. This is not to say that partisan contacts and social structural influences are immaterial to their initial involvement, but the focus of this paper is necessarily upon the activation process during the campaign, and primary voters did not need to be activated.

If party contacting is immaterial to general election turnout among primary voters, does it have any effect upon the two-party division of the vote? The bottom of Table 6 shows that, at least in terms of the presidential vote, the contacting coefficients for party contact lie in the substantively appropriate directions but fail to satisfy the 95% level of confidence. A very different situation occurs with respect to the gubernatorial and congressional elections. In three of four instances, the coefficients not only lie in the expected directions but also satisfy at least the 95% level of confidence. Both of these elections produced highly visible campaigns. The incumbent Republican governor took advantage of a significant statewide organization to ensure his reelection; and South Bend's popular and well-known Democratic county prosecutor mounted an aggressive but unsuccessful challenge against the incumbent Republican congressman.[8] These results, which show an effect due to party activity in subnational elections, are in keeping with a stream of research documenting the importance of parties and party activities particularly at the state and local level (Caldeira, Clausen, and Patterson 1990; Caldeira, Patterson, and Markko 1985; Katz and Eldersfeld 1961; Patterson and Caldeira 1983; Wolfinger 1963).

How large are the estimated effects due to party contacts? Table 7 uses coefficient estimates from the bottom of Table 6 to illustrate the effect of party contact upon the predicted probability of a Republican gubernatorial vote, across categories of party identification. The effect of the partisan contact is especially pronounced among independents and Republicans. This is not to argue that contacting is always important, only that it can be. And these results suggest that it is more likely to be important in state and local contests, among the citizens who participated in party primaries.

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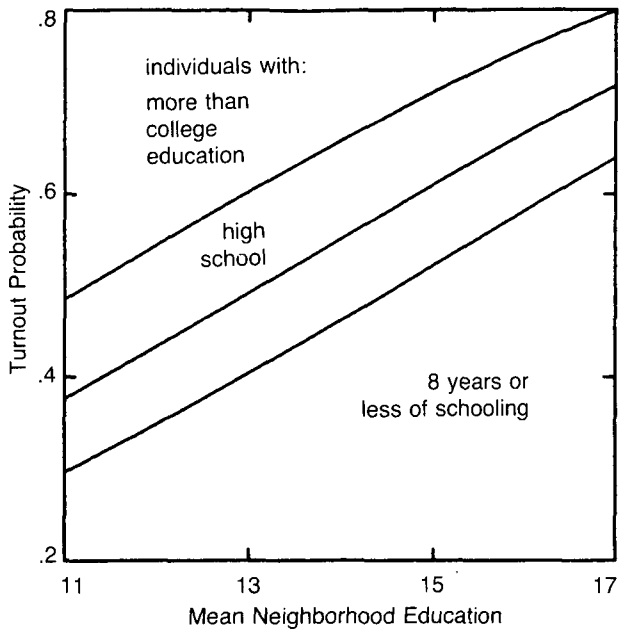
Figure 3

Turnout Probability in the General Election by Individual and Neighborhood Education, for Nonvoters in the Primary Election



Note: Age is held constant at 45 years; neighborhood proportions contacted by Democrats and Republicans are held constant at .10; partisanship is held constant at *independent*; and Republican and Democratic contact are held constant at *no contact*. Based on estimates from Table 6.

Thus, the irony of party contacting deepens. In terms of turnout, politically marginal citizens (i.e., who failed to vote in the primary election) appear to lie beyond the reach of partisan organization. Conversely, politically engaged citizens (who vote in party primaries) are overwhelmingly likely to vote in the general election *whether they are contacted or not*. The effectiveness of organized partisan effort is most pronounced among citizens who vote in the primaries, but the effect lies in altering the two-party division of the vote in state and local contests. Thus, it would appear that partisan contact is less important as a vehicle to get out the vote than as a means of swaying the preferences of voters who are already likely to vote.

We could end the analysis here with the conclusion that party contacting only has an effect on the direction of the vote among some citizens in some contests. But does such an analysis exhaust the possibilities? Are we looking in the right places to understand the consequences of partisan mobilization? Should we only view the party canvass as a direct instrument of mass mobilization enlisting voters one by one to the party's cause? Or should we also view the canvass as a catalytic agent that stimulates voter mobilization by enlisting additional party workers in the effort?

OTHER FORMS OF ELECTORAL INVOLVEMENT

Voting in an election is only one form of electoral involvement that is susceptible to the influence of party mobilization efforts. A whole range of gladiatorial activities are central to the electoral process and dependent upon party organizational activities. We consider four of these activities here, based upon the following questions asked in the third, postelection wave of the survey.

1. Did you work for any candidate in this election?
2. Did you go to any political meetings, rallies, dinners, or things like that?
3. Did you put up a political yard sign or bumper sticker during the campaign?
4. Did you give any money to a political party or candidate?

Each of these activities considered singly is, of course, a rare event when compared with the frequency of voting. But taken together they point toward involvement that is perhaps surprisingly widespread. A summary index is formed by counting the number of activities in which each respondent reported participating. A score of zero indicates no involvement in any of the activities (71% of the third-wave respondents), and a score of four indicates involvement in each of the four activities (2% of the third-wave respondents). Thus, nearly 30% of the

TABLE 7

Predicted Republican Proportion of Two-Party Vote for Governor in General Election among Primary Voters, by Party Contact and Party Identification

PARTY IDENTIFICATION	CONTACTED BY			
	DEMOCRATS	REPUBLICANS	NEITHER	BOTH
Dem. identifier	.04	.15	.08	.07
Rep. identifier	.65	.89	.81	.77
Independent	.25	.59	.44	.38

Note: All other variables are held constant at intermediate values: age = 45 years, educ. = 12 years, nghbd. educ. = 12 years, nghbd. Dem. contact = .10, nghbd. Rep. contact = .10. Based on estimates from the bottom of Table 6, col. 3.

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TABLE 8

Partisan Electoral Involvement As a Function of Various Factors, Including Party Contact

EXPLANATORY VARIABLES	COEFFICIENT	t-VALUE
Constant	−.24	.77
Dem. identifier[a]	.24	4.38**
Rep. identifier[a]	.21	3.42**
Voted in Dem. primary[a]	.07	1.25
Voted in Rep. primary[a]	.30	3.48**
Dem. contact[a]	.24	4.08**
Rep. contact[a]	.19	3.26**
Dem. contact in nghbd.[b]	−.44	1.22
Rep. contact in nghbd.[b]	1.19	2.92**
Indiv. educ.[c]	.04	3.98**
Nghbd. educ.[d]	−.02	.86

Note: Least squares model. N = 1,439; R^2 = .08.
[a]Dummy coded.
[b]Proportion contacted.
[c]Years of school.
[d]Mean school years.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

sample reported engaging in at least one of these activities.

As Table 8 suggests, the structure of political life looms large with respect to these other forms of electoral involvement. In particular, party contacts play an important role; but there is also evidence to suggest that partisan identification, primary participation, and the level of party contacting within neighborhoods (at least, by Republicans) may make a difference, as well.[9] In contrast, while the effect of individual education is substantial, the neighborhood educational level has no effect at all. Unlike the case of general election turnout among nonvoters in the primary, political structure goes much farther than social structure in explaining levels of activation and involvement.

SECOND-ORDER CONSEQUENCES OF THE PARTY CANVASS

The party canvass has substantial consequences for activating the activists, but does it serve to stimulate a more widespread mobilization process? One opportunity to address this question lies in examining the consequences that derive from simple, grassroots partisan activity—displaying a political yard sign or bumper sticker. Does the aggregate distribution of yard signs and bumper stickers have any measurable consequence for the individuals who are exposed to them? At the second wave interview, during the heart of the campaign, we asked our respondents a question regarding which presidential candidate they thought would win the most votes in their neighborhood. We also asked whether they thought it would be a close race, so that a four-point scale can be constructed measuring the respondents' expectations regarding voting behavior among neighborhood residents.

How might partisan activity alter these expectations? In Table 9, column 1, the expectation regarding the neighborhood is regressed on respondents' preelection candidate preferences and the neighborhood distribution of yard signs and bumper stickers; and the results show that the distribution of yard signs and bumper stickers is quite clearly related to neighborhood expectations. The problem is, of course, that while Democratic yard signs and bumper stickers *do* tend to be located in Democratic neighborhoods, they are not the only ways in which residents of a neighborhood form judgments regarding the political tendencies of other residents. We have no method for sorting out the extent to which signs and stickers are responsible for these judgments, but we can examine the extent to which respondents' expectations are misinformed—the extent to which yard signs and bumper stickers have effects that are not attributable to the actual distribution of support.

The respondents' expectations are considered once more in Table 9, column 2; but controls are included for the *actual* distribution of votes in the respondents' neighborhoods. Our strategy is to reconsider yard signs and bumper stickers but to subject their effect upon respondent perceptions to a control for reality. Do visible manifestations of support continue to affect respondent perceptions once we simultaneously take into account the reality upon which the perceptions are based? It must be emphasized that this is a very stringent test. Our new question becomes, Do yard signs and bumper stickers have an effect upon perceptions of support when we control the actual levels of support upon which the perceptions are based? Thus, we are ignoring yard signs and bumper stickers as vehicles for communicating the reality of these support levels and focusing instead upon their potential to produce alternative perceptions of the same underlying reality.

This new test is accomplished by aggregating the reported votes of respondents from the third-wave, postelection survey within each of the neighborhoods. Treating the resulting measures as the actual distributions of support within the neighborhoods, we are able to isolate the independent informational effect due to visible demonstrations of support—the yard signs and bumper stickers. A single measure for the distribution of signs and stickers has been constructed providing the difference in the proportions of neighborhood residents displaying Democratic versus Republican yard signs and stickers (proportion Democratic minus proportion Republican). As the table shows, this variable measuring the distribution of yard signs and bumper stickers produces a statistically discernable effect with an appropriate sign for neighborhood expectations.

Contextual theories of politics often assert that citizens surrounded by a particular preference are more likely to adopt the preference. The empirical support for such an argument is substantial and widespread (e.g., Segal and Meyer 1974). But the

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TABLE 9

Expectations of Election Outcomes in Neighborhood by Respondent's Preference and Neighborhood Yard Signs and Bumper Stickers, with and without Controls for Actual Levels of Support in the Neighborhoods

EXPLANATORY VARIABLES	FOR ACTUAL SUPPORT LEVELS IN NEIGHBORHOODS: NO CONTROLS	CONTROLS
Constant	3.41 (32.63)**	2.24 (8.28)**
Mondale supporter[a]	−.41 (4.97)**	−.36 (4.51)**
Reagan supporter[a]	.15 (1.91)	.11 (1.42)
Dem. signs and stickers[b]	−22.54 (14.13)**	—
Rep. signs and stickers[b]	8.48 (10.75)**	—
Nghbd. Mondale vote[b]	—	−1.92 (3.26)**
Nghbd. Reagan vote[b]	—	2.77 (8.34)**
Partisan diff., signs and stickers	—	−4.11 (4.88)**
R^2 =	.29	.36
Number of cases	939	939

Note: Ordinary least squares model. Coefficient t-values are in parentheses.
[a]As is the case with the dependent variable, these dummy variables are measured at the second wave of the survey, prior to the general election. Only the neighborhood variables are taken from the third, postelection wave of the survey. Only respondents participating in both the second and third waves are included in this table.
[b]Neighborhood proportion.
[c]The partisan difference in signs and stickers is measured as the neighborhood proportion with Democratic signs and stickers minus the neighborhood proportion with Republican signs and stickers.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

TABLE 10

Individual Party Identification As a Function of Neighborhood Partisanship and Other Individual-level Factors

EXPLANATORY VARIABLES	FOR PRESIDENTIAL VOTE CHOICE: NO CONTROLS	CONTROLS
Constant	−.68 (1.55)	.09 (.24)
Indiv. educ.[a]	.10 (3.60)**	.08 (3.29)**
Family income[b]	.15 (4.11)**	.09 (3.00)**
Age	.01 (2.08)*	.01 (2.75)**
Ethnic attachment[c]	−.45 (3.99)**	−.30 (3.18)**
Union member in family[c]	−.71 (5.54)**	−.25 (2.28)*
Protestant[c]	.90 (7.95)**	.63 (6.56)**
Nghbd. party ID[d]	.36 (3.89)**	.26 (3.34)**
Mondale voter	—	−1.66 (14.12)**
Reagan voter[c]	—	1.31 (11.47)**
Number of cases	1,326	1,285
R^2	.20	.46

[a]Years of school.
[b]Range, 1–8.
[c]Dummy-coded.
[d]The respondent's own party identification is excluded in the calculation of the neighborhood mean for that respondent.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

enduring critique of such an argument is an alleged failure to document the mechanisms whereby the aggregated preferences of a population are translated into a source of influence for individuals (Hauser 1974). We supply one part of the response to this critique: an important mechanism of contextual influence is party effort. People know their neighbors' politics, in part because of party organization aimed at informing them.

Viewed somewhat differently, party organization is designed to exploit social organization. Table 10, column 1, displays a regression of party identification among the South Bend respondents on a range of different measures: education, income, age, the strength of the respondent's ethnic attachment,[10] union membership, religion, and the partisan composition of the respondent's neighborhood. As this regression shows, individual partisan orientations are subject to the influence of the individual's partisan surroundings, even when we include controls for a wide range of individual level characteristics (Hauser 1974). In particular, the mean partisanship of the neighborhood has important consequences for the partisanship of individuals who live within the neighborhood. Even when we take account of short-term factors affecting partisanship (Franklin and Jackson 1983) by controlling for presidential vote choice in Table 10, column 2, the effect of the partisan context on individual loyalties is still substantial. In short, individual partisanship is contingent upon partisan surroundings. Party organization is not the only mechanism that ties individual preference together with the politics of the larger community (Huckfeldt and Sprague 1987, 1991); but it certainly is one.

These data demonstrate that party activity matters. The seemingly ordinary act of putting up a yard sign sends a message to others who are exposed to the sign. But the yard sign is only one part of a more complex, highly structured process of political mobilization. Organizations make contact with potential

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activists who, in turn, make contact with the population at large. Huge amounts of slippage occur along the way. Organizational contacts often fall on deaf ears; or, still worse, misdirected contacts serve to mobilize supporters of the other party. But the significance of the contact does not end once the contact is made. Rather, the contact serves as a catalyst that sets into motion a series of events that in the end has important consequences for ordinary citizens—concerned bystanders who pass judgment on the electoral contests of democratic politics.

CONCLUSION AND IMPLICATIONS

How should we view the efforts made by party organizations to mobilize the electorate? Most importantly, as long as we focus primarily upon the direct individual consequences of party contacts for voting behavior, we will continue to see the party as playing a marginal role in U.S. politics, and we will consistently underestimate the consequences of party activity. Kramer's (1970–71) work on party contacting deservedly became a watershed event in the analysis of party mobilization efforts; but it also led to a subtle but important redefinition of the expected consequence that derives from party activity. An earlier tradition of analysis (perhaps represented best by Katz and Eldersveld 1961) addressed the aggregate pattern of party activity and its consequence for the aggregate distribution of party support. Kramer's analysis turned toward an individual-level analysis of these phenomena, an approach that was readily embraced by social scientists with a heightened sense of skepticism regarding any aggregate data.

The problem remains: partisan mobilization is best understood at two levels of analysis and meaning—the individual and the aggregate. In the general context of U.S. politics, it is unreasonable to expect that party contacts should produce dramatic consequences for the voting behavior of those individuals who are contacted. Rather, party contacts are most important as a means of mobilizing potential activists. Most people do not give money and put up yard signs unless someone asks them to do so; thus, we see a catalytic function of party activity. Party organizations mobilize the faithful, and the activity of the faithful sends a message to the rest of the public.

It is ironic that political scientists have become so skeptical regarding party activity and its consequences. If party activity is so ineffective, why do parties and candidates continue to invest their resources in the activity? Gibson and his colleagues (1983, 1985) investigate the vitality of party organizations at both the state and local levels and show that state and local party organizations started to become more active during the 1960s and 1970s. Krassa uses national survey data to show that the incidence of party contacting has generally increased during the post–World War II period (1985, 5–6). It is possible, of course, that academics understand something that activists have missed; but in this instance it is doubtful.

There are, of course, limits upon the ability of party organizations to mobilize the electorate; and the limits are more pronounced today than they were 50 years ago. The effectiveness of Mayor Daley (the elder) and his precinct runners and precinct captains was genuinely impressive; but organizations such as these have always been the exception rather than the rule. Absent such an organization (which presents an impossible standard of effectiveness), we should not assume that political organization is absent.

Finally, the biggest challenge to any political organization is strategic—developing methods of mobilization that are efficient and effective. As we have seen, numerous obstacles inhibit this sort of effectiveness; and the majority party is perversely vulnerable to its own special strategic constraints. The lesson to be drawn from this analysis is that the parameters determining the relationships between parties and voters, and thus determining the success of partisan mobilization, are environmentally contingent. They cannot be understood from afar.

Notes

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1. As a matter of law, Indiana voters are not entitled to take the primary ballot of a political party unless they are supporters of that party. Indeed, voters are subject to challenge if they request the ballot of a party they do not support. As a practical matter, such challenges are virtually nonexistent; and few people (including the officials who are responsible for conducting primary elections) are even aware that they might be issued. Thus, voters *might* request different primary ballots at different elections.

2. The text of the contacting question was, "As you know, the political parties try to talk to as many people as they can to get them to vote for their candidate. Did anyone from one of the political parties call you up, or come around and talk to you about the campaign this year? Which party was that?"

3. For example, two-thirds of the respondents who voted in the 1984 Democratic primary report voting for Walter Mondale; and 94 percent of the respondents who voted in the 1984 Republican primary report voting for Ronald Reagan.

4. All measures of the vote used in this paper are validated with registration data. Respondents who do not appear on the registration list are counted as nonvoters. Respondents who appear on the registration list *and* who reported voting for Mondale or Reagan are counted accordingly.

5. A methodological possibility is that partisanship works well for the Republicans because much of partisan identification is a simultaneous function of vote choice. This alternative interpretation seems unlikely in light of the very strong education effect for Republican contacts. Whatever else this class measure may proxy, it is certain that vote does not determine education. Substantive, rather than methodological, interpretations are more persuasive.

6. In order to determine whether a respondent displayed a Democratic or a Republican yard sign or bumper sticker, their presidential vote is used as an indicator. For example, we assume that self-reported Reagan voters who also report displaying a yard sign or a bumper sticker did in fact display a Republican sign or sticker. They might, of course, have

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displayed a sign or a sticker for a Democratic candidate running for some other office. Indeed, in view of the election results, where many Democratic identifiers voted for Reagan, we probably overestimate Republican signs and stickers and underestimate Democratic signs and stickers. The resulting bias is not likely to compromise our analysis to any serious extent. Split-ticket voters are less likely to display signs and stickers; thus, the bias is not likely to be profound. Indeed, these data show that (using our measure) less than 1% of self-identified Democrats put up a Republican sign or sticker and that not a single self-identified Republican put up a Democratic sign or sticker. Alternatively, none of the Democratic signs or stickers were put up by Republicans; and only 6% of the Republican signs and stickers were displayed by Democrats.

7. In light of work showing reciprocal causation between vote choice and party identification measured as a seven-point scale (Franklin and Jackson 1983), we have restricted our measure of partisanship to three categories: Democratic, Republican, and independent (the baseline category). This practice is based on the assumption that people are much more likely to shift the strength of their attachment within categories than they are to switch their actual attachment during the campaign—an assumption that is sustained by the South Bend data. For a more detailed discussion of these issues relative to the South Bend study, see Huckfeldt and Sprague 1991.

8. The incumbent congressman, John Hiler, was one of House Speaker Tip O'Neill's "Reagan robots," originally winning office in the 1980 election. He had only narrowly hung on to his seat in the 1982 midterm election, but in 1984 he took advantage of Reagan's coattails to win more easily (Mondak 1990).

9. An alternative analysis of these data was undertaken in which electoral involvement was truncated to a binary variable: those who had participated in *any* of the activities versus those who had participated in *none* of the activities. This variable was, in turn, the criterion variable in a logit regression with the same explanatory variables as in Table 7. The results were wholly consistent with the Table 7 analysis.

10. The strength of a respondent's ethnic attachment is measured with the question, "How important would you say your ethnic background is to you?" Possible responses are *very important* or *somewhat important* (coded one) and *not too important* (coded zero).

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THE STRATEGIC AGENDA IN LEGISLATIVE POLITICS

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We examine the politics of the strategic agenda. Abstracting a politics on the liberal–conservative dimension, we analyze Key Vote roll call data from the U.S. House of Representatives during the Eisenhower and Reagan administrations. The data suggest that politicians set the policy agenda in a strategic fashion. Because they consider such factors as long-term political goals, the changing institutional setting, and plebiscitary presidential politics, agenda-setters propose legislation that only imperfectly reflects their and the membership's wishes on the issue at hand. Thus, as the final stage in the political process, the strategic selection of an agenda provides a means by which factors other than policy preferences affect policy outcomes. The analyses affirm the strategic agenda as a core element in political life.

During the Summer of 1982, in response to mounting public concern over the nuclear arms race, Liberals in the U.S. House of Representatives proposed an immediate Nuclear Freeze (HJ Res 521). Conservatives coalesced around a counter-proposal that called for a reduction in nuclear weaponry to parity, thus endorsing the Reagan position that the U.S. weapons deficit would have to be erased in any arms control agreement. During a fortnight of debate and politicking, the Liberals discovered that they needed to make concessions to Moderates in order to attract votes. Accordingly, they accepted amendments that would weaken the link to SALT II, emphasize the goal of establishing nuclear equivalence with the Soviet Union, encourage the pursuit of arms reduction, and provide for on-site verification. Conservatives considered blocking these "sweeteners" in the hopes that the original proposal would be unacceptable to Moderates. They ultimately decided to refrain, largely because their supporters found the provisions appealing "on the merits." In the end, the Liberal concessions proved inadequate when the House adopted the Conservative substitute by a razor-thin 204–202 margin.

In the next Spring, Freeze proponents sponsored a similar resolution (HJ Res 13), this time agreeing to some 30 additional amendments to attract the support of Moderate and Conservative members. (The amendments allowed U.S. weapons buildup and further missile deployments prior to formal treaty ratification, provided for weapons modernization for deterrence or for "safety," exempted missile-launching submarines and aircraft, made on-site inspection and verification a *sine qua non*, linked the freeze to subsequent arms reductions, and conceded that the resolution did not compel Presidential action.) Having thus isolated the opposition by conceding almost every substantive point, the Freeze leadership prevailed 278–149. *During subsequent news conferences, both sides claimed victory.*

—(Adapted from *Congressional Quarterly Almanac*)

When we understand the politics of a particular contest, we need know the stakes, as well as the outcome. Often as not, the way that the alternatives are defined determines the result. Accordingly, practicing politicians make the definition of the alternatives (the political agenda) an integral part of their political strategy. Moreover, that strategy (and thus the agenda and the policy outcome) will naturally reflect considerations other than preferences about the policy matter at hand. It is in this sense that we wish to posit—and illustrate—the importance of the strategic agenda in legislative politics.

That agendas are strategic[1] has important implications. First, those who initiate proposals (agenda-setters) will tailor the policy content to have a chance to win. Thus, because the stakes of the game are free to be defined by the players, the nominal outcome of a legislative battle (who wins, who loses) masks, rather than reveals, matters of real consequence.

Second (and more significantly), the strategic character of agendas indicates the importance of the strategies politicians choose. These strategies will vary, perhaps in complicated ways, as politicians pursue different goals, consider institutional constraints, and take advantage of political opportunities; that is to say, factors independent of policy content will affect the way the agenda is set. From the point of view of democratic theory, the crucial result is that the connection between policy and preferences will depend on how politicians craft agendas to serve their own purposes.

We shall show that legislative agendas are strategic. To do this we build an empirical model with parts taken from a theoretical view of agenda politics. Each component generates a different empirical test, which we then confront with roll call data. The evidence reveals that factors other than policy preferences affect the way politicians formulate policy proposals and thus that preferences do not govern outcomes. The fundamental result affirms the strategic agenda as a core element in political life.

A STYLIZED VERSION OF LEGISLATIVE POLITICS

Consider first a single dimension for political conflict, the liberal–conservative continuum. While this characterization of politics may have little to do with logic or philosophy, it has the advantage of being recognizably dominant by politicians and political observ-

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ers (Kritzer 1978). Because it represents the major way of seeing political coalitions and conflict, our focus on it as the political battleground should move us forward in understanding how legislative politics are structured. To be sure, much of what goes on cannot be characterized in such simple liberal–conservative terms. We shall be constantly aware that other factors may affect political strategy and political action. However, for purposes of analytic leverage, we shall keep such factors in the background, as unspecified.

The problem is a classic. Say that a liberal legislative committee wishes to propose a health care policy for a floor vote in the House of Representatives. How should the committee blend factors such as cost, accessibility, scope, and so forth? In our stylized one-dimensional space, how liberal or how conservative should the mixture be? When the committee expects the House to ratify the committee recommendation, then the answer is trivial—as liberal as the committee wants.

Things get more interesting when House action becomes political. The committee must trade off its preferences for a liberal policy content against the prospects of losing a floor vote and diminishing its political reputation; its choice will depend on the mixture of goals the committee seeks. It further must consider the institutional rules and norms and patterns of behavior that affect its ability to define the issue and to build a supporting coalition, as well as the opposition's willingness and capacity to organize behind counterproposals. Finally, the committee will want to think about the possibility that outside actors (notably the president) will enter the fray, change the scope of conflict, and transform the political situation. These sorts of factors will affect the committee's proposal—the agenda on which the membership is to vote. The calculus is strategic.

If we want to tell whether agendas are truly strategic, we need empirical data on agendas, and we need to know whether they are a function of such factors as politicians' goals, institutional structure, and political opportunities. We turn to data.

THE EMPIRICAL ESSENTIALS

To study agenda politics, we require a set of political issues that attracted enough political attention to merit the sorts of strategic thinking in which we are interested. We should also like to have issues that were sufficiently controversial to generate an actively engaged opposition so that we might observe strategic politics in a contested field. Finally, we should like to identify the particular roll calls on which the issue was actually decided (perhaps a procedural vote), rather than content ourselves with votes on either trivial amendments or on the sometimes consensual final passage. In short, we seek evidence on politics in its most sophisticated form.

We have chosen to analyze the politics of a set of *Congressional Quarterly*'s Key Votes. Of course, no solution will be perfect. Nevertheless, by selecting these items, we concentrate on issues that produced politics of white heat, that commanded the attention of the national press and of all political actors in Washington (Shull and Vanderleeuw 1987). These are not parochial fights for pork or turf. Relying on the staff of *Congressional Quarterly* means that we are trapped in their definition of what constitutes a crucial political test or a major national issue. Yet it is just the contemporary judgment of such keen observers that enables us to select issues that satisfy our need.

In the end, we have analyzed the politics of the Key Votes taken during the Eisenhower and Reagan administrations. Superficially, both the 1950s and the 1980s were characterized by popular conservative Republican presidents opposed by an organized and savvy Democratic House leadership. Importantly, then, these two eras provide us with sustained examples of divided government. Apart from the obvious contemporary interest in such government, the pattern allows us to observe strategic interactions when both congressional parties had a sharp interest in the politics of governing rather than in the politics of opposition. Further (and crucial for our work), the two periods are politically distinct. First, inside the House, we observe political patterns from very different eras, ranging from the concerted oligarchy of Sam Rayburn to the decentralized brokerage of Tip O'Neill. Second, we observe a contrast in the extent to which presidents mobilized public support in order to seek legislative success (compare the relative indifference of Eisenhower to the energetic efforts of Reagan). Thus, our choice of periods produces a contrast in both internal and external politics while maintaining similarity in the overall lay of the political land.

Further, we have limited our analytic frame to a single policy dimension.[2] At the cost of representative accuracy, this decision allows us to examine a large number of political issues. Having multiple cases proves crucial for our work because we intend to test, rather than illustrate, theoretical propositions.

In making this decision, we understand that we shall miss much of real substantive interest and that much of what we say will appear simplistic to the practitioner's eye. We surely appreciate the complexities of politics, the subtleties of the art. Our reading of the legislative histories here confirms our natural respect for the ability and inclination of politicians to play sophisticated, not to say Byzantine, politics. But we are also convinced that our representation captures, albeit incompletely, an integral part of everyday politics. Just as the nuclear freeze proponents and opponents calculated the sorts of policy concessions and maneuvers that might enhance their chances of success, so do most politicians most of the time. We suspect that the setting of the legislative agenda is neither idiosyncratic nor casual; we suspect that it is systematic and strategic.

We have chosen to concentrate on the single dimension employed by the Americans for Democratic

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Action (ADA) in their annual ratings of individual members of Congress. We use ADA, rather than (say) the American Conservative Union (ACU), because their ratings extend to the beginnings of our time frame. This ADA dimension corresponds nicely with the liberal–conservative dimension that so dominates dimensional analyses of roll call voting (Poole and Daniels 1985; Poole and Rosenthal 1987; Weisberg 1978). Further, its terms are readily recognized by practicing professionals and correspond to representatives' and staff members' subjective judgments about the political landscape (Smith, Herrera, and Herrera 1990).

We gain empirical purchase here by scoring individual legislators in terms of their "era-specific" ADA ratings, that is, their average ratings during the Eisenhower or Reagan years.[3] The ADA score represents a member's voting tendency (in liberal–conservative terms); it is a measure of regularized voting patterns. As such, the member's ADA score reflects the combination of factors that establish his or her standing voting decision, including the political composition of the home district, the long-term influence of Washington-based political forces, and (of course) personal ideology.

We are thus able to identify interesting political battles and to define a common policy space that is immediately understandable. Further, both the selection of cases and the definition of the policy space are exogenously determined; that is to say, we take politics on its own terms (as understood in the Washington community) at the very outset.

MEASURING THE DECISIVE AGENDA

The main empirical difficulty lies in measuring just how far bill sponsors have gone in trading off the substantive content of the proposed legislation against the prospects of victory. We need a consistent measurement of the extent to which agenda-setters have trimmed their sails or raised their spinnakers.

We use a simplified version of the Poole–Rosenthal algorithm (Poole and Rosenthal 1985). The idea is based on a straightforward spatial model: each proposed bill (or amendment) may be represented by a point in our liberal–conservative domain. Similarly, the alternative (the current status quo or whatever legislators see as the likely outcome if the proposed bill fails) may also be so represented. A line (or, more generally, a plane) bisects the distance between the proposal and the alternative to provide an *indifference point*.

By way of example, consider the top line of Figure 1. Here a liberal committee has proposed a bill at 50 on a 0–100 ADA scale. The alternative lies at 40; thus, the indifference point is 45. Any legislator whose ideal point lies at 45 (a moderate conservative) will be indifferent between the bill and the alternative. All legislators to the liberal side will prefer the bill, while all those to the conservative side will prefer the alternative.

In real life, of course, legislators do not obligingly conform to academic theory. Let us say that a legislator at 60 votes *nay* on the roll call. Such an individual will have voted "unexpectedly" for any number of reasons. Perhaps the legislator's preferences are poorly measured by ADA, perhaps the bill's implications are ambiguous, perhaps the decision involves policy dimensions other than liberal–conservative, perhaps side payments are involved, or perhaps the legislator simply makes a mistake. In our health care example, a liberal legislator may choose to vote *nay* after meeting a delegation of health insurance lobbyists and a conservative may vote *yea* because the new policy would open a clinic in his home district.

The empirical question is, Can we uncover the policy mixtures of the bills having only the roll call records? With the data at hand, we think the placement of bill and alternative beyond our reach. However, we *can* get a clean measure of the indifference point. When the sponsor of the bill alters its policy content, he or she simultaneously moves the indifference point on the liberal–conservative continuum. More directly, this indifference point is the politically decisive part of the equation.

Consider the example again. If every legislator voted according to our unidimensional theory, then uncovering the indifference point would be easy. Observing the idealized voting pattern in line 2 of Figure 1, we would simply follow along the liberal–conservative scale until we discovered the point at which legislators shift from *yea* to *nay*.

The actual data, of course, look much more like line 3 of Figure 1. Here, several members have chosen to vote in ways not predicted by the one-dimensional abstraction. Given these aberrations, we must develop an "error theory." Here, we simply assume that these extra considerations (having to do with lobbying and partial payoffs) are minimal; that is to say, we choose the indifference point that minimizes the "errors" associated with a liberal–conservative voting model, the one that requires the least amount of extraneous politicking. Technically, we employ an iterative estimation procedure that searches the policy space for the indifference point minimizing the amount of "error" in voting.[4] More fundamentally, we produce an indifference point estimate by assuming that all voting is governed by liberal–conservative considerations plus minimal unspecified disturbances. In the end, we are able to estimate the Key Vote indifference points within about two ADA points on the hundred-point scale.

EVIDENCE ON STRATEGIC AGENDAS: AN EMPIRICAL MODEL

Given the data, we need an empirical model of agenda politics that is grounded in a theoretical view. For this purpose we seek a powerful yet spare representation of congressional agenda strategy. Step by step, we shall construct and test our model using four

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Figure 1

One Dimensional Voting and Estimating Indifference Points

A Schematic Setup

```
Conservative                                                    Liberal
                        Alternative          Bill
  0 ____________________________|____________|_____________________ 100
                                |
                                |
                          Indifference
                             Point
```

The Theoretical Voting Pattern

```
Conservative                                                    Liberal
                                  +
                                  +
  0        nnnnnnnnnnnnnnnnnnnnnnnnnnnnnn+yyyyyyyyyyyyyyyyyyyyyyyyyyyyyyyyyyyy  100
                                  +
                                  +
                             Estimated
                           Indifference
                               Point
```

A Typical Voting Pattern

```
Conservative                                                    Liberal
                                  +
                                  +
  0        nnnnnnnnnnnnnnnnnnnnnYnnnYnYYYn+yyyyyyNyNyyyNyyyyyyyyyyyyyyyyyyyyy  100
                                  +
                                  +
                             Estimated
                           Indifference
                               Point
```

elements taken from established theory: (1) the dominance of the median voter equilibrium, (2) the goals of the political actors, (3) the institutional setting in which contests take place, and (4) the effect of the president's actions on political opportunities. We begin with a model of what occurs when agendas are not truly strategic.

The Null Model: The Median Voter Equilibrium

The most elementary part of our model incorporates preference distributions into agenda formation. Of course, the legislators' individual preferences, by themselves, do not necessarily define a collective preference. In fact, much theoretical work (notably, that inspired by Arrow 1963) has shown the inherent difficulty in translating individual choices into collective choices. Thus, no matter how clever, legislators cannot pick policy proposals that will be sure to win a majority vote (McKelvey 1976).

Nevertheless, legislators press on, unencumbered by general theory: they work with models that work "well enough." We, too, wish to proceed using a model that captures reality "well enough." We concentrate on politics played *mainly* on a one-dimensional field, one that runs from the liberal to the conservative pole. Since such a politics incorporates Black's (1958) assumption of single-peaked preferences, it yields a powerful theoretical prediction: majority voting produces the outcome preferred by the median voter. This basic result is most famously illustrated in Downs's (1957) spatial model of party platforms and party competition and in Riker's (1962) theory of minimal winning coalitions.

In our bare-bones model, we expect that politics on the House floor will center around the median House member; that is to say, congressional agenda-setters, intuitively understanding Black's result, will propose policies that attract the support of the median member. Consider liberal agenda-setters. If they should nominate a "sincere" liberal proposal, they will find that the opposition will quickly introduce a (winning) counterproposal to the right of the median.[5] A single-play agenda contest thus punishes agenda-setters who start with sincere positions.

To be sure, contests over the agenda are not single-play but will include proposal, counterproposal, countercounterproposal, and so on. In theory,

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agenda-setters who propose policies far from the median will generate a sequence of successful amendments, each closer to the median member's preference. In practice, agenda-setters will anticipate such moves and propose policies already near the expected final result. The implicit sequential game converges quickly.[6]

The question is, Do real politicians take this logic into account? Figure 2 presents the graphic evidence on the point. The vertical dimension represents the passage of time (sequencing the key votes) and the horizontal dimension shows the ADA scale (0 being the most conservative and 100 the most liberal). The vertical hashes in the center of the diagram represent the median member's policy preference (in ADA units) for each Congress. Each circle represents the indifference point for a given bill. Note that the placements do center around the median House member positions. Note also the substantial scatter.

Clearly, the policy preference of the median member changed from one Congress to the next. The rise and fall of each party's electoral fortunes affected the policy preferences of the House as a collectivity. The biggest shifts come from the Democratic victories associated with the recessions of 1958 and 1982. Liberal Democrats replaced conservative Republicans, and the changeover moved the center of the political landscape to the left.

This historical movement provides us with analytic leverage, as we may see if the bill placements change with the median preference. When the House moves to the left, so should the placements of proposals. Table 1 gives the first-cut empirical evidence: the first column presents the regression of each bill's indifference point on the ADA score of the median House member at the time of the bill's introduction. Included are 158 bills from the Eisenhower and the Reagan years. The F-test ($p = .01$) clearly indicates that political actors considered the changing composition of the House when they chose their agenda strategies. The coefficient estimate (.39) indicates that bill sponsors adjusted to 39%[7] of the change in the median preference. The median voter, or minimal winning coalition, expectation is confirmed.

The overall fit disappoints. However, agenda-setters do not always manipulate their policy proposals toward a winning position. Sometimes, because they see longer-run political advantages or perhaps because they believe their policy proposal must not be compromised, agenda-setters may "stick to their guns." For example, just before the 1954 congressional elections, the Democratic Leadership backed an enhanced-benefits amendment to the administration's unemployment compensation bill (a straight New Deal move), knowing full well that they had no chance at victory. In 1959 the Eisenhower administration pushed a raise in the ceiling for long-term Treasury bonds (private long-term rates were above the ceiling, and the government could not borrow), again knowing that they had no realistic chance. In both cases, the agenda-setter knew that the proposal could not pass but pursued the course nevertheless.

Our median voter model should operate most powerfully when both sides "play the game." Consider, instead of the entire set of roll calls, those that were closely contested, say, those decided by margins of 80 or fewer votes (a vote of 250 to 170 or closer).[8] For these competitive votes the median voter model does considerably better: note the rightmost column in Table 1. Now the agenda-setters "adjust" to 58% of the change in the median preference. Further, the simple model now "accounts for" 22% of the variance in indifference point placement. Clearly (in closely contested political battles at least) agenda-setters incorporate the collective distribution of preferences into their strategic calculations.

If congressional politics produced only median voter outcomes, then we should merely cite Black (1958) and close up shop. Obviously, we observe outcomes that suggest we consider additional theoretical factors.

The Mixture of Objectives

The elementary median voter result ignores the agenda-setters' motives. To move forward, we introduce two complications: (1) we allow members' preferences for policy proposals to be affected by factors other than liberal–conservative policy, and (2) we permit political actors to pursue multiple goals when they propose legislation.

The unidimensionality assumption is too restrictive. Instead, we posit that any individual member's preference over policy proposals is defined *primarily* by the proposals' placements on a liberal–conservative dimension but also by other considerations. Here, we wish to be vague. These other considerations might include policy factors that cannot be properly termed liberal or conservative, or they may include matters extraneous to the content of the legislation. In general, we shall refer to these as *side payments*, meaning to include all inducements that might cause members to change their votes on a given matter. In our model, this factor has the effect of introducing uncertainty into the calculus: a voter's preference is a function of liberal–conservative policy plus unknown, unspecified factors.

More interesting, members of Congress have different types of career objectives and choose agenda strategies accordingly. Legislators seek victory in the electoral arena, formulate "good public policy," and pursue personal political power (Fenno 1973, 1978).[9]

When proposing legislation, agenda-setters must choose. On the one hand, they might put forward a policy that reflects an ideal point (perhaps a personal preference but more likely a policy that has political payoffs); on the other hand, they might pick a policy that has a maximum chance of winning a majority vote. Politicians pursue different strategies to pursue different goals. (For convincing evidence, see Poole and Smith 1986.) Yet the trade-off is not easy.

Agenda-setters motivated primarily by electoral politics, provocateurs, will emphasize symbols over substance. Concerned with the broad array of public

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Figure 2

Bill Indifference Points

Eisenhower Administration

Date	Subject	Indif. Point
01APR53	TIDELANDS OIL STATES	52.29
22APR53	PUBLIC HOUSING	40.16
20MAY53	SOIL CONSERVATION	38.07
02JUL53	AIR FORCE	40.78
09JUL53	PRIVATE POWER-NIAGARA FALLS	51.82
31JUL53	MUTUAL SECURITY	35.20
18MAR54	TAX CUT/DIVIDENDS	37.16
02APR54	HOUSING AID	39.73
08APR54	WIRETAPPING RESTRICTIONS	32.03
30JUN54	FOREIGN AID	27.04
02JUL54	FARM PRICE SUPPORTS	44.71
08JUL54	UNEMPLOYMENT COMPENSATION	53.22
24JUL54	ATOMIC ENERGY + PATENTS	40.09
18FEB55	RECIPROCAL TRADE EXTENSION	46.12
25FEB55	INCOME TAX CUT $20	41.35
05MAY55	FARM PRICE SUPPORTS (RIGID)	43.42
10MAY55	STATEHOOD ALASKA HAWAII	48.46
27JUL55	HIGHWAY GAS TAX	60.92
28JUL55	NATURAL GAS DEREGULATION	41.59
29JUL55	HOUSING ACT	46.41
01MAR56	COLORADO RIVER PROJECT	36.23
11APR56	PRICE SUPPORTS (RIGID)	39.19
27JUN56	VETERANS PENSIONS	29.44
05JUL56	SCHOOL CONSTRUCTION	48.46
06JUL56	POSTAL RATES	47.14
11JUL56	FOREIGN AID	29.30
24JUL56	ATOMIC POWER	43.49
18JUN57	CIVIL RIGHTS JURY TRIAL	38.11
18JUN57	CIVIL RIGHTS PASSAGE	32.62
25JUL57	SCHOOL CONSTRUCTION	46.54
15AUG57	FOREIGN AID	63.92
20MAR58	FARM TOBACCO SUBSIDY	40.79
01MAY58	UNEMPLOYMENT COMPENSATION	50.82
14MAY58	FOREIGN AID	35.24
28MAY58	ALASKA STATEHOOD	40.47
05JUN58	ARMY BUDGET	40.14
11JUN58	TRADE AGREEMENT	39.38
12JUN58	REORGANIZE DOD - RECOMMIT	42.39
17JUL58	FEDERAL PREEMPTION LIMIT	53.58
01AUG58	PUBLIC FACILITIES	45.28
08AUG58	EDUCATION NDEA	34.33
07MAY59	TVA BOND	40.50
24JUN59	FEDERAL PREEMPTION LIMIT	52.97
13AUG59	LANDRUM-GRIFFIN <SUBSTITUTE>	52.94
27AUG59	HOUSING ACT	22.64
04SEP59	SAVINGS BONDS RATES	31.02
23MAR60	CIVIL RIGHTS VOTING REGISTRATION	29.88
18MAY60	FOREIGN INVESTMENTS TAX	52.27
26MAY60	SCHOOLS + POWELL AMENDMENT	46.78
17JUN60	FOREIGN AID	44.00
23JUN60	FARM AID - WHEAT	59.52
30JUN60	MINIMUM WAGE	47.73
23AUG60	CONTEMPT CITATION - COLT	31.71

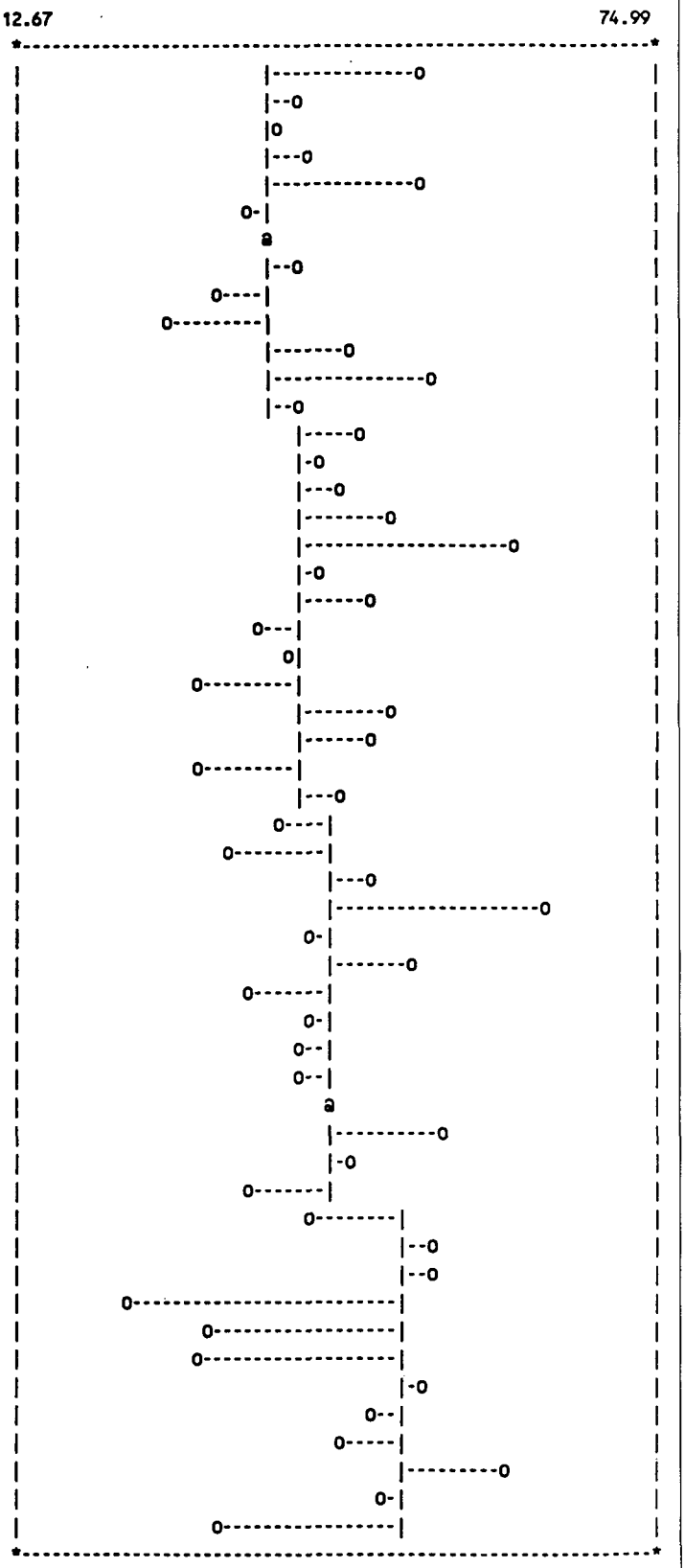

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Figure 2

Bill Indifference Points (Continued)

Reagan Administration

Date	Subject	Indif. Point
07MAY81	BUDGET LATTA SUBSTITUTE	55.22
18JUN81	LEGAL SERVICES	20.49
26JUN81	RECONCILIATION - LATTA	42.20
23JUL81	TOMBIGBEE	42.83
24JUL81	CLINCH RIVER	41.58
29JUL81	TAX CUT	49.00
31JUL81	SOCIAL SECURITY	57.57
05OCT81	VOTING RIGHTS ACT	19.54
06OCT81	HHS, EDUCATION, LABOR	26.44
14OCT81	AWACS TO SAUDI ARABIA	18.55
18NOV81	B-1 BOMBER	62.53
18NOV81	MX MISSILE	59.09
11DEC81	FOREIGN AID	36.74
16DEC81	FARM PRICE SUPPORTS	41.64
12MAY82	HOUSING MORTGAGE SUBSIDY	15.29
27MAY82	MEDICARE AND DEFENSE	37.26
10JUN82	BUDGET - LATTA SUBSTITUTE	41.29
22JUL82	CHEMICAL WEAPONS	35.34
28JUL82	BUDGET RECONCILIATION	43.22
05AUG82	NUCLEAR FREEZE - START	40.50
29SEP82	SOVIET SANCTIONS	44.07
01OCT82	BALANCED BUDGET AMENDMENT	47.63
06DEC82	HIGHWAY & TRANSIT	42.61
07DEC82	MX MISSILE	28.23
14DEC82	CLINCH RIVER	36.73
14DEC82	RADIO LIBERTY & VOA	38.63
15DEC82	DOMESTIC CONTENT - AUTOMOBILES	40.11
16DEC82	CONTEMPT CITATION - GORSUCH	20.73
09MAR83	SOCIAL SECURITY	53.90
23MAR83	BUDGET RESOLUTION 1ST	44.82
04MAY83	NUCLEAR FREEZE	26.12
11MAY83	HOME LOANS & HOMELESS	47.62
28JUL83	CONTRA AID	45.36
27SEP83	COAL PIPELINE	44.92
28SEP83	LEBANON FORCE - STAY	57.23
01NOV83	MX MISSILE	52.36
08NOV83	EDUCATION APPROPRIATIONS	36.74
09NOV83	DAIRY PRICE SUPPORTS	45.87
10NOV83	TELEPHONE ACCESS CHARGE	31.37
15NOV83	CHEMICAL WEAPONS	32.75
02FEB84	CHILD ABUSE	53.95
20MAR84	DAM PROJECTS	48.16
05APR84	BUDGET RESOLUTION 1ST	41.76
12APR84	BUDGET RECONCILIATION	40.53
10MAY84	EL SALVADOR AID	52.54
23MAY84	ASAT WEAPON	41.05
24MAY84	CONTRA AID	39.89
31MAY84	MX MISSILE	49.39
26JUL84	SCHOOL PRAYER	42.04
09AUG84	SUPERFUND LAWSUITS	51.74

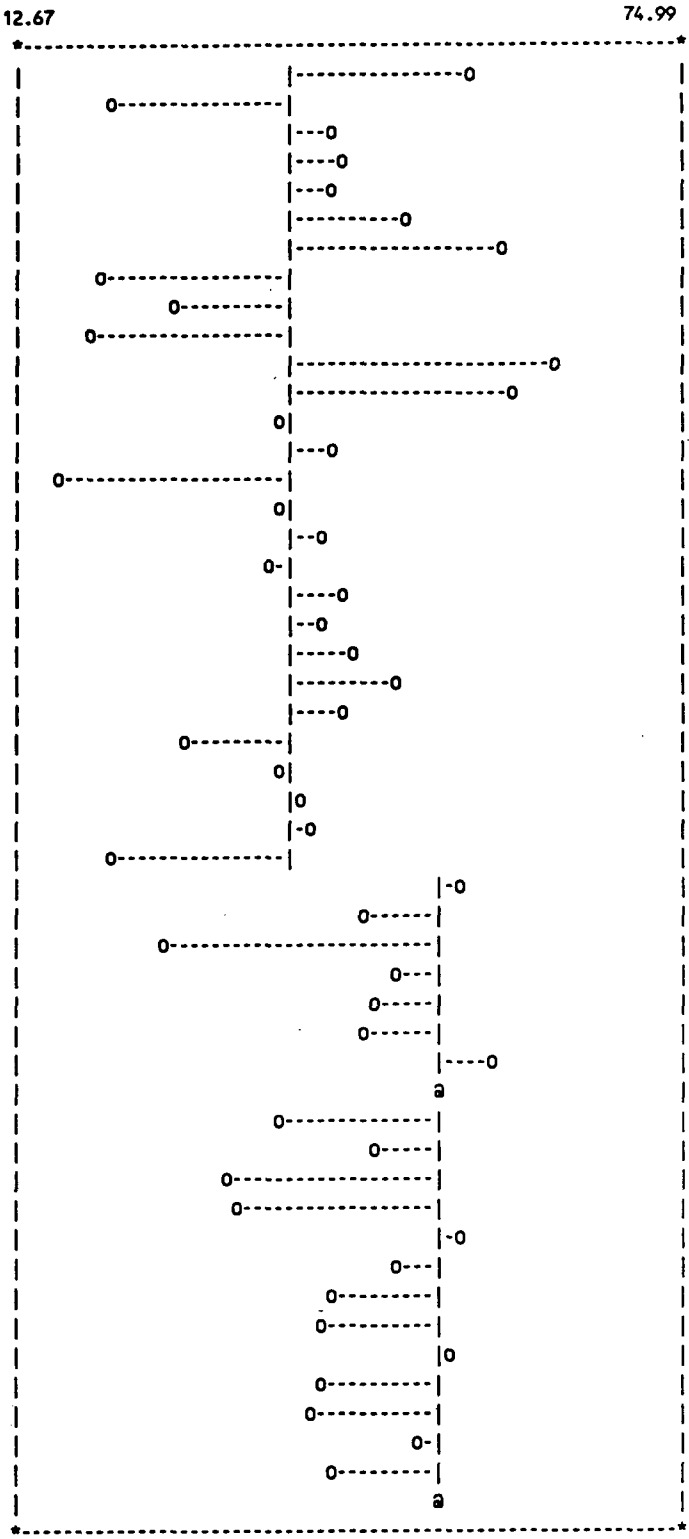

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Figure 2

Bill Indifference Points (Continued)

Reagan Administration

Date	Subject	Indif. Point
25SEP84	APPROPRIATIONS ADD CRIME	63.02
03OCT84	GEPHARDT AMENDMENT	60.76
11OCT84	SOUTH AFRICA LOANS	12.67
26MAR85	MX MISSILE	47.80
30APR85	INDIANA 8TH DISTRICT	44.96
05JUN85	SOUTH AFRICA ANTI-APARTHEID	17.45
12JUN85	CONTRA AID (BOLAND AMENDMENT)	53.52
19JUN85	CHEMICAL WEAPONS	48.66
20JUN85	STAR WARS	50.75
10JUL85	ANGOLA - REPEAL CLARK AMENDMENT	57.82
03OCT85	FARM PROGRAM REFERENDUM	58.88
10OCT85	TEXTILE IMPORT QUOTAS	40.34
24OCT85	BUDGET RECONCILIATION - LATTA	46.58
01NOV85	DEBT LIMIT	36.87
21NOV85	PLANT CLOSING	46.98
10DEC85	SUPERFUND TAX BUSINESS	44.00
10APR86	GUN CONTROL EASE	66.53
05JUN86	PUBLIC HOUSING	55.95
25JUN86	CONTRA AID	49.75
06AUG86	SPEED LIMIT 65	46.99
08AUG86	DEFENSE BUDGET	31.72
08AUG86	NUCLEAR TEST BAN	34.62
12AUG86	STAR WARS	35.93
15AUG86	DEBT LIMIT & GRAMM-RUDMAN	34.05
17SEP86	ANGOLA REBEL AID	54.75
19SEP86	EPA REGULATE PESTICIDES	51.38
25SEP86	TAX REFORM	34.43
25SEP86	CONTINUING RESOLUTION (DEFENSE)	49.77
01OCT86	CHEMICAL WEAPONS	44.22
18MAR87	SPEED LIMIT 65	50.40
09APR87	BUDGET RESOLUTION	45.60
23APR87	BUDGET CUT - ACROSS BOARD	61.17
29APR87	GEPHARDT AMENDMENT	55.55
19MAY87	NUCLEAR TEST BAN	39.76
08JUL87	REFLAG KUWAIT SHIPS - DELAY	40.34
22JUL87	CATASTROPHIC HEALTH INSURANCE	42.68
30JUL87	PRICE-ANDERSON ACT	54.96
21OCT87	INDEPENDENT COUNSEL LAW	36.02
29OCT87	BUDGET RECONCILIATION	55.42
03DEC87	DELAY CLEAN AIR - EPA	38.45
15DEC87	WELFARE REFORM	51.23
03FEB88	CONTRA AID	48.15
23MAR88	BUDGET RESOLUTION	27.82
21APR88	PLANT CLOSINGS & TRADE	35.32
28APR88	NUCLEAR TEST BAN	45.02
04MAY88	STAR WARS	33.22
02JUN88	CATASTROPHIC HEALTH INSURANCE	17.61
22JUN88	APPROPRIATIONS - NASA, HUD, EPA	59.08
23JUN88	HOUSING DISCRIMINATION - CHILDREN	24.57
28JUN88	ABORTION D.C.	56.56
07JUL88	WELFARE REFORM	61.25
04AUG88	JAPANESE AMERICANS REPARATIONS	32.41
08SEP88	DRUGS DEATH PENALTY	75.00
15SEP88	HANDGUN CONTROL WAIT	49.80

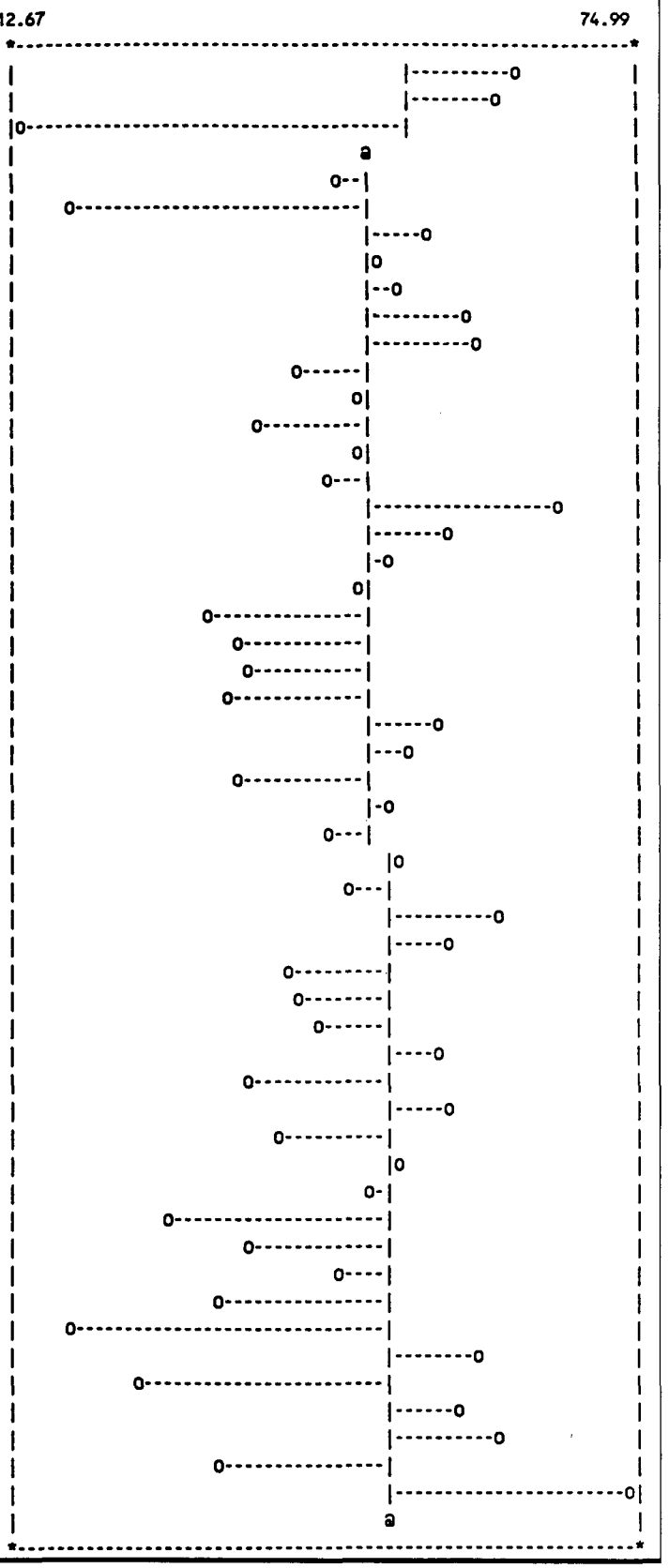

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TABLE 1

Bill Indifference Point As a Function of Median Preference of Current Congress

VARIABLE	ALL ISSUES COEFFICIENT	CLOSELY CONTESTED ISSUES[a] COEFFICIENT
Median preference[b]	.39	.58
	(.16)	(.11)
Constant	25.55	19.13
	(7.25)	(4.92)
R^2	.04	.22
Adjusted R^2	.03	.21
F-value	6.12	29.49
p value for equation	0.01	0.00
Number of cases	158	107

Note: The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Standard errors are in parentheses.
[a] These include only the roll calls decided by 80 votes or less.
[b] The ADA ideal point of the median member for each Congress.

opinion and understanding the power of symbolic gesture (Edelman 1971), such actors will take great care to shun compromise and instead frame political choices in stark, dramatic, unrealistic, and perhaps confrontational terms. When electoral politics dominate actors' minds, we expect agenda-setters to ignore their chances of winning on the floor and instead to concentrate on taking the high ground.[10]

Next, legislators who seek primarily to shape the contours of public policy will engage in persuasion, compromise, and bargaining. In such legislators' minds, a median preference proposal represents no gain at all, and a "sincere" proposal of liberal or conservative purity will be unrealistic. Instead, these actors seek the support of potential opponents: they compromise, they promise future political support, they introduce side payments. Limited by their resources, such legislators produce proposals shaded toward—but not fully realizing—their own preferences.

Finally, many legislators (and certainly the party leaderships) act primarily to maintain and enhance their own political power. (For a theoretical discussion, see Calvert 1987.) The pursuit of political power, and equivalently the appearance of political power, requires that actors gain political successes: legislators understand the sense that weakness begets weakness and strength begets strength (Neustadt 1960). Effective leaders must not only produce policies but claim credit for those policies.

The presence of uncertainty leads to a seemingly perverse result. The initiating legislator will know the preferences of other actors, including the median member, only with uncertainty: Policy matters involve value trade-offs that are complex, side payments make the voting preferences of legislators unpredictable, and so on. Thus, the agenda-setter can only guess about the median position. This fact has a different effect on those shaping policy and on those seeking power.

The *policy shaper* knows that the "null" model implies a victory for the true median preference position. If the liberal actor proposes a policy slightly to the left of Center, the actor will have two chances of victory: (1) the bargaining and side payments may attract enough would-be opponents to pass the legislation, and (2) the true median position may be to the left of the actor's guess. The downside of proposing liberal legislation is that the result will be the median preference position—neither loss nor gain.[11] The policy shaper is thus risk-neutral and perhaps risk-acceptant.

Unlike the symbolic provocateur and the policy shaper, the *powerseeker* must avoid losing the nominal vote and is thus risk-averse. Knowing the median preference only with uncertainty, the liberal legislator will introduce proposals *more* conservative than the median. Thus, those mindful of winning will trade off policy content for prospects of victory.

As a first cut, consider the possibility that the final agenda-setter of a bill (or amendment) has a strategic advantage over the opponent. By making the motion, the agenda-setter makes the decision to contest an issue on a particular substantive mix. From the historical record we are able to determine the individual actor who moved the bill. Most often, these actors' objectives are transparent, either in form of final passage or of an amendment to weaken or kill a piece of legislation.[12]

In a free-floating, unstructured, political situation the agenda-setter has no advantage because the opposition may nominate another alternative that will approximate the median voter outcome more closely. Yet House floor politics are hardly free-floating. Obviously, House rules allow only limited opportunities for repeated considerations. In most of our cases, the agenda-setter of a bill (or amendment) served as a *final* agenda-setter (in the sense of Romer and Rosenthal 1978); that is, the Key Votes we analyze were considered at the time as the decisive vote.

Figure 2

Bill Indifference Points (Continued)

←

Note. The indifference points for each bill represent the point on the liberal-conservative scale where the legislators shift from *yea* to *nay*. They are graphed (small circles) on the 0–100 ADA scale, running conservative to liberal. Conceptually, each represents the ideal point of the legislator who is indifferent between the proposed bill and the expected alternative. The vertical hash marks represent the ADA score of the median member for each congress. The estimates are produced by an iterative estimation procedure that searches the policy space for the indifference point minimizing the amount of error in voting. For a more explicit and technical statement, see n. 5.

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TABLE 2

Bill Indifference Point (Deviation from Median Voter) as a Function of Agenda-setter

AGENDA-SETTER	COEFFICIENT
Conservative	1.44
	(1.37)
Liberal	−5.29
	(1.31)
R^2	.08
Adjusted R^2	.07
F-Value:	6.69
p value for equation	.00
Prob. that conservative and liberal agendas are same	.00

Note: Standard errors for the coefficients are in parentheses. The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Note that this metric means that a positive move is toward the liberal pole and that a negative move is toward the conservative pole. The agenda-setter variable reflects the sponsor of the bill and whether or not the bill received the backing of the party leadership (for Democrats) and of the party leadership and the president (for Republicans). N = 158.

TABLE 3

Bill Indifference Point (Deviation from Median Voter) as a Function of Agenda-setter Leadership or Maverick

AGENDA-SETTER	COEFFICIENT
Conservative leadership	4.63
	(1.90)
Liberal leadership	−7.32
	(1.41)
Conservative maverick	−1.71
	(1.66)
Liberal maverick	.99
	(2.27)
R^2	.18
Adjusted R^2	.16
F-Value:	8.26
p value for equations	.00
Prob. that conservative maverick and leadership agendas are same	.04
Prob. that liberal maverick and leadership agendas are same	.00

Note: Standard errors for the coefficients are in parentheses. The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Note that this metric means that a positive move is toward the liberal pole, and that a negative move is toward the conservative pole. The agenda-setter variable reflects the sponsor of the bill and whether or not the bill received the backing of the party leadership (for Democrats) and of the party leadership and the president (for Republicans). The party leadership here is indicated by the unanimous positive support of both the majority (or minority) leader and the whip (plus the president for the Republicans). Unsupported moves are termed *mavericks*. N = 158.

It is theoretically instructive to observe distinctions between liberal and conservative placements. If actors merely push their own preferences, then liberals should nominate bills with indifference points to the left of the median voter (produce a positive score in ADA terms), while conservatives should place theirs with indifference points to the right (a negative score). If, instead, the actors value victory more than policy, they will choose indifference point placements to the opposite side of the median preference (liberals to the right, conservatives to the left).[13] When these motives are neatly balanced, the median voter outcome will dominate; we shall not gain any empirical leverage.

Table 2 provides statistical estimates of how liberal and conservative agenda-setters choose to place their bills' indifference points *relative to the median voter*.[14] Both liberals and conservatives were inclined to concede on policy in return for political success. Liberals set the agenda 5.29 ADA points rightward of the median preference, conservatives 1.44 points to the left. The distinction between liberals and conservatives is statistically discernible and of modest magnitude. It indicates that agenda-setters consider power, as well as policy outcomes.

This first cut, however, includes moves by all sorts of members in either coalition. Importantly, the analysis mixes the initiatives of backbenchers and renegade committees with the more calculated moves of the two coalitions' leaderships. If we are to take full advantage of asymmetries in motivations, then we should separate the *mavericks* (for whom symbolic outcomes may be most important) from the political professionals (who must consider both the importance of achieving genuine policy outcomes and the importance of winning for political power's sake).

Table 3 makes just this distinction. Here, we distinguish the moves of liberal or conservative mavericks (that is, agenda-setters who operate independently from their political leaders) from the strategies of the leaderships.[15] The pattern looks surprisingly satisfying. For mavericks, we observe only a modest difference between liberals and conservatives. Pointedly, these differences reflect modest symbolic gratifications, with liberals making moves slightly to the left (.99 ADA points) of the median voter, conservatives slightly to the right (−1.71). On the other hand (and most importantly), the political leaderships evince a distinctive inclination to trim their policy objectives in an effort to obtain political victory. The Democratic party leadership initiates action substantially to the right of the median preference (−7.32), while the Republican leadership (when representing the presidential administration) initiates action to the left (4.63). In the name of political success, these moves concede considerable policy to the opposition.

The Institutional Setting

Much of what political science has learned about legislative politics indicates that institutions (the sets of rules and norms that govern behavior and expec-

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tations of behavior) play a central role when preferences get turned into policies. In a general theoretical sense (Riker 1980), rules and norms effectively dominate politics. In our simplified agenda contest, institutions are important because they govern the management of uncertainty. Politicians will concede policy goals (in the pursuit of success) when they are uncertain about outcomes and thus need to build a "margin of safety" into what might otherwise be a minimal winning coalition.

Clearly, technical rules matter. Agenda-setters have an enormous advantage when the opponents' cannot propose amendments (McKelvey 1976; Romer and Rosenthal 1978). Further, when rules such as *germaneness* constrain tactics, strategic calculations take on more order (Shepsle 1979). Often, the combination of rules and available strategies will produce "structure-induced" equilibria.[16] Yet as the structural setting ranges from "closed" to "open," uncertainty rises—and so do considerations of risk.

At the same time, informal norms affect uncertainty. When the membership adheres to traditional institutional guidelines such as *apprenticeship, specialization, reciprocity,* and *committee deference,* then agenda-setters can expect few floor maneuvers in opposition to committee-sponsored legislation. Junior members refrain from floor activity and senior members defer to their colleagues' judgment. When those norms disappear, then more members with different objectives will participate, and uncertainty will accordingly rise.

Finally, shared understandings about the exercise of internal political power shape uncertainty. When political power is organized hierarchically and when non-policy-based sanctions are concentrated in the formal leadership structure, then the number of "independent" political actors will be restricted. When political power is widely dispersed, then the number of actors and interests of whom coalitions must be formed will increase. Commensurately, uncertainty will rise.

Thus, our theory predicts that institutional structures affect politicians' strategies in the agenda game. Under complete information, politicians may safely maximize policy gains by creating minimal winning coalitions. However, when institutions change to increase uncertainty, then we expect that politicians will trade away policy to ensure political success. For a test, we take advantage of the large-scale historical change that characterized the House between the 1950s and the 1980s.

In the days of Rayburn, McCormack, Martin, and Halleck, Congress watchers saw a highly structured politics. Committees did not only the House's work but its politicking as well. The substance of politics was often determined by close negotiations between Rayburn and the committee chairs. Members subscribed to restrictive norms (such as apprenticeship, committee deference, and so on). Finally, the formal leadership (most notably the senior committee chairs) could and did exercise internal sanctions to keep junior members in line.

This stable order was upset by revolutionary change. Starting in the late 1960s and early 1970s, members took on a more individualistic bent, becoming *managers of enterprises* (Salisbury and Shepsle 1981) and *policy entrepreneurs* (Loomis 1988). The old order of "go along, get along" died a quick death. The new breed of members altered the face of House politics to empower individual members at the expense of the formal hierarchy. This fundamental transformation left the House drenched in uncertainty (Shepsle 1989; Smith 1989).

The changes in House politics dictated a change in leadership styles. O'Neill and Wright, taking advantage of the reforms of the 1970s, gained—and began to exercise—control over the formal rules.[17] Perhaps more telling, they adopted a more inclusive and extensive leadership that attended to the interests and policy objectives of the growing number of policy entrepreneurs (Sinclair 1989). Coalition formation began to emphasize a coincidence of policy interests rather than the mutual accommodation that characterizes the maintenance of a governing coalition.

Thus, our agenda-setting prediction depends on comparing the 1980s' *decreased* uncertainty (due to the leadership's exercising control over the formal House rules) with the 1980s' *increased* uncertainty (due to the leadership's need to organize an increasingly atomistic membership). Given our reading of the anecdotal evidence, we suspect that the 1980s were characterized by *more*, rather than *less*, uncertainty. Our empirical work will provide a test.

Our theory says that institutional change influences agenda-setters because it affects uncertainty. When the strategists can reasonably expect to define the ground over which the floor fight will take place, then they might reasonably cut the margin razor-thin. For example, if the roll call were to be decided on liberal–conservative terms, then a concession of two ADA points would yield a margin of about 15 votes (producing a final total of, say, 225 to 210). Yet, the data in Table 3 suggest that the leaderships, on average, calculated safety margins *two to three times* that amount.

Given our understanding of the different political worlds of the 1950s and 1980s, we should expect that the eras' strategists would have planned accordingly; that is to say, while trading off between policy and political success, Rayburn's Democrats might have allowed much smaller "margins of safety" than the Democrats lead by O'Neill and Wright could afford.

The data in Table 4 confirm our expectation. The conservative leaderships maintained a rough margin of safety in both eras (4.78 and 4.56 points). The liberals (Democrats) clearly cut things dramatically closer in the 1950s than in the 1980s (compare −1.68- with −10.28-ADA-point concessions). The Democratic leaders, under Rayburn, were able to manage House politics to their advantage. The disappearance of restrictive norms, the decentralization of political power, and the rise of policy entrepreneurship forced O'Neill and Wright to turn toward policy concessions in order to assert their political leadership.[18] If this

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TABLE 4

Bill Indifference Point (Deviation from Median Preference) As a Function of Agenda-setter by Political Era

AGENDA-SETTER	1953–60	1981–88
Conservative leadership	4.78	4.56
	(3.09)	(2.33)
Liberal leadership	−1.68	−10.28
	(2.29)	(1.74)
Conservative maverick	1.88	−2.12
	(2.56)	(2.10)
Liberal maverick	−1.07	1.60
	(4.58)	(1.74)
R^2	.06	.24
Adjusted R^2	.00	.22
F-value	.70	10.48
Number of cases	54	104
Prob. that conservative maverick and leadership agendas are same	.48	.04
Prob. that liberal maverick and leadership agendas are same	.91	.00

Note: The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Note that this metric means that a positive move is toward the liberal pole, and that a negative move is toward the conservative pole. The agenda-setter variable reflects the sponsor of the bill and whether or not the bill received the backing of the party leadership (for Democrats) and of the party leadership and the president (for Republicans). The party leadership here is indicated by the unanimous positive support of both the majority (or minority) leader and the whip. Unsupported moves are termed *mavericks*. Standard errors are in parentheses. The coefficients are the mean indifference point generated by each type of agenda-setter.
* $p = .79$.

simple test is a guide, structure (and lack of structure) sets the bounds of agenda strategy.

Plebiscitary Presidents and Political Opportunity

The institutional changes in Congress comprise part of a broader array of changes in U.S. politics. Polsby (1978) suggests that the key lies in an erosion of the traditional means of "intermediation." Political actors now emphasize mass media persuasion, rather than interpersonal bargaining: confrontation has replaced negotiation. Agenda politics is out in the open.

Presidential politics has been in the vanguard of this revolution. Presidents commonly assert—and the public apparently agrees—that only the presidency stands for national interests and can assume the lead in national policymaking. Accordingly, presidents have come to adopt strategies in which they mobilize mass political support to alter the complexion of congressional politics. In the current style, presidents strategically "go public" (Kernell 1986) to alter the political opportunities for legislative success. Straightforwardly, when they mobilize public support, presidents are able to enhance the chances of their policy proposals.

Of course, the mechanisms here may vary. For example, when the president gives a national television address and generates thousands of supportive letters, moderate Democrats may act *as though* the president's conservative bill is, in fact, moderate. In this case, they anticipate that their constituents will see the bill as acceptable when otherwise, they would not. Of course, the president need not actually convince anyone. The president may give speeches, with legislators as his real audience—to give members pause because they cannot be sure that their constituents are unmoved. This is a classic case of the third-party effect in communications (Davison 1983). Alternatively, when the president energizes public support or when he is evidently "popular" in the electorate, legislators may accept the president's program simply because no single individual is willing to stand up and criticize.

A quick test of this idea may be seen by adding three variables to the agenda-setter model, the first measuring presidential approval and the second and third indicating whether the president went public on the issue (taking a conservative or a liberal position).[19] If plebiscitary politics affects Congress directly, we expect approval and conservative speechmaking to move the indifference point in a liberal direction (that is, public pressure would make moderately liberal members indifferent to a bill that they would ordinarily oppose) and liberal speeches to move the indifference point in a conservative direction. These expectations are not sustained. The additions are *not* statistically significant for the full set of data ($p = .75$), nor are they significant for either the Eisenhower or the Reagan eras ($p = .38$ and $p = .65$).[20] At first blush, plebiscitary politics has little influence on congressional agendas.

Yet things may not be so direct. Suppose agenda-setters take presidential activity into account before deciding on the policy mixtures to offer. Conservative agenda-setters will calculate the expected effects of presidential speechmaking into their sense of what is possible to achieve, perhaps adding a little more "conservative tonic" into the policy mixture. Likewise, when faced with an active and popular president, liberal agenda-setters will water down their proposals.

This effect of presidential activity on agenda-setter behavior depends on the precision of politicians' expectations about presidential effects. Under perfect information, when the effects of presidential activity are known with certainty, agenda-setters may exactly balance the effects of presidential activity against the policy trade-offs they might otherwise make: we should observe policy proposals that *apparently* center around the median member position.

Things get more interesting when agenda-setters cannot be sure about how much influence the president wields. Legislators' uncertainty about the effects of presidential activities, like legislators' uncertainty about the median voter position, allows us to detect differential weighting of objectives. Because they cannot be sure about the outcomes, agenda-setters must balance policy gains against political success. If

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their trade-offs are consistent, leaning in one direction or the other, then we should see a pattern in the agendas they offer.

Plebiscitary politics should affect the nature of political opportunity and thus, indirectly, political agendas. When the president is popular, his supporters should have a greater chance of "winning." Further, when the president goes public, he also raises the stakes of the game, in effect signaling a *must win* situation. Agenda-setters should respond to the changing political situation by altering their strategies.

Conservatives may pursue policy gains that might otherwise be unobtainable: they would then err on the side of proposing bills that end with the indifference point on the conservative side of the median preference. On the other hand, conservatives may emphasize politics and "play to win"—mainly when the president enjoys popular support or goes public, these being their only realistic chances for victory. Of course, when conservatives seek victory, they must make policy concessions to ensure success; they must maneuver toward the liberal side of the median House member. Thus, our expectation for conservatives depends on how plebiscitary politics affects the relative importance of policy and power.

Similarly (though not symmetrically), the changing political circumstances should affect the liberals' strategies. When the president rides high or mobilizes public support, we should expect liberals (both policy shapers and powerseekers) to produce proposals even more conservative than they otherwise might.

Thus, if the effect of plebiscitary politics on congressional proposals is not direct but, instead, indirect—influencing not preferences but, instead, agendas—we should observe differently signed effects for conservatives and liberals, respectively. This we find to be true. In simple bivariate regressions, we find that a president's going public moves conservative agenda-setters to concede seven ADA points to the left and moves liberal agenda-setters to concede seven ADA points to the right. Significantly, both conservative speeches and liberal speeches produce the same effect (a concession to the opposition), indicating that the important part of going public is that it changes the stakes of the political contest. Similarly (though less consistently), approval also produces strategic concessions on the part of both conservatives and liberals.

Understanding that plebiscitary politics produces approximately equal and opposite effects, depending on the agenda-setter, we may examine its power to augment our agenda-setter–institutional change model. In Table 5 we introduce two "composite" variables[21] for approval and going public, signed to match our expectation that presidential activity and popularity will generate agenda-setter concessions. In the context of the full model, we observe that agenda-setters concede an average of about one-fifth of an ADA point (−.22) for each point change in presidential approval and about five ADA points (−5.09) whenever the president goes public. The numbers pass statistical muster, with p-values of .10 and .06 individually, .05 jointly.[22] Thus, we may say that plebiscitary politics matters.

TABLE 5

Bill Indifference Point (Deviation from Median Preference) As a Function of Agenda-setter and Plebiscitary Presidential Politics

VARIABLE	COEFFICIENT[a]
Going public–agenda-setter interaction[b]	−5.09 (2.71)
Presidential approval–agenda-setter interaction[b]	−.22 (.13)
Conservative leadership	2.88 (2.58)
Liberal leadership	−6.72 (2.16)
Conservative maverick	−.68 (1.57)
Liberal maverick	1.21 (2.76)
R^2	.19
Adjusted R^2	.16
F-value	7.08
Prob. of no effect of	
Going public	.06
Approval	.10
Going public and approval	.05

Notes: The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Note that this metric means that a positive move is toward the liberal pole and that a negative move is toward the conservative pole. The agenda-setter variable reflects the sponsor of the bill and whether or not the bill received the backing of the party leadership (for Democrats) and of the party leadership and the president (for Republicans). The party leadership here is indicated by the unanimous positive support of both the majority (or minority) leader and the whip. Unsupported moves are termed *mavericks*. Standard errors are in parentheses. N = 158.

[a] As before, the agenda-setter coefficients are the mean indifference point generated by each type of agenda-setter. Those associated with going public and approval variables show the amount of *additional concession* associated with the president's going public or with each point change in public approval.

[b] These terms represent agenda-setter (1 = liberal, −1 = conservative) × going public (1 = *yes*, 0 = *no*) and approval (percentage points deviation from the president-specific mean).

Further, it matters just as conventional wisdom expects. In separate analyses for the 1950s and the 1980s, we observe that public presidential politics became more important in the latter period. The joint tests produce statistical tests of .28 and .05, respectively. However, we observe relatively equal power for going public during both eras (it induces strategic concessions of about five to six ADA points). The main difference is that presidential popularity played practically no role in the 1950s, while it surely did in the 1980s.

The overall picture is clear: politicians adapt their agenda strategies to changing political opportunities. When presidents go public, they raise the *political*

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TABLE 6

A Comparison of Components by Political Era: Bill Indifference Point As a Function of Median Preference, Agenda-setter, and Plebiscitary Politics (Closely Contested Issues)

MODEL COMPONENTS	1953–60	1981–88
Median preference[a]	.00	.00
Agenda-setter[b]	.55	.03
Plebiscitary politics[c]	.71	.06
Agenda-setter and plebiscitary politics	.54	.00
R^2	.48	.36
Adjusted R^2	.38	.31
F-value:	4.95	7.62
Number of cases	33	74

Note: The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. These include only those roll calls decided by 80 or fewer votes. Each entry is the probability that the component, or combination of components, is not statistically discernible.

[a] The ADA ideal point of the median member for each Congress.

[b] Dummy variables for liberal leadership or for conservative leadership. Mavericks are not differentiated.

[c] Interactive variables for going public and for approval combining with the camp (liberal or conservative) of the agenda-setter.

stakes of the game and thus encourage power-sensitive agenda-setters to build in increasingly large margins of safety. Further (in recent years, at least), when presidents enjoy popular support, they and their legislative allies "play to win." In effect, when presidents activate the mass public, they alter the character of politics: they encourage politicians to trade away policy objectives in pursuit of raw political power.

A Comparison of Model Components by Political Era

The evidence for a transformation in strategic agenda-setting becomes striking when we put all the parts together. Table 6 incorporates three empirical components (the median preference, the agenda-setter, and plebiscitary politics) in separate equations for each of our two political eras (represented in the two columns). Here, we analyze only the closely contested issues (those with margins of 80 or fewer votes), so that we may reintroduce the median preference into our consideration (see Table 1). Table 6 summarizes the statistical power of each model component (rather than of the separate variables within each component). They are partial F-tests and give the probability of each component's being statistically insignificant. Thus, we may judge the applicability of the median preference, the agenda-setter, and the plebiscitary politics components before and after the changes of the 1960s and 1970s.

Compare the two eras. In the 1950s, when the House was relatively orderly and Eisenhower made sparing use of television, agenda politics closely followed the House median preference. The effects of the agenda-setter and plebiscitary politics components are statistically insignificant ($p = .54$). Notably, the median preference component, by itself, accounts for more of the indifference point variance for the 1950s (48%) than does the full model for the 1980s (36%). In contrast, when Reagan was president, the median preference was important but did not dominant: half of the "explained variance" is uniquely attributable to the agenda-setter and plebiscitary politics components. The party leaderships carefully crafted their proposals to pursue political power and to reflect rise and fall in presidential politics. Thus, the dramatic historical changes, both in the institutions that govern House politics and in the character of external politics, have fundamentally transformed the strategic calculus that governs the political agenda.

CONCLUSIONS

We now know that the connection between legislative policy and members preferences depends on how politicians craft agendas. Produced by such elements as the career goals of the actors, the institutional setting, and the structure of political opportunity, the strategic agenda transforms options and preferences to shape legislative outcomes. As the final stage in the political process, the strategic agenda acts as a camera lens that produces meaningfully different results as its focus is manipulated.

Four empirical tests support this view. First, strategic behavior appears at the most elementary level. Our examination of the median voter model indicates that political actors do indeed consider the changing composition of the House when they choose agenda strategies. Second, goal trade-offs affect strategic behavior. Concerns with political power produce agenda strategies that contrast sharply with those associated with policymaking and symbolic political goals. Third, structure constrains strategic behavior. In the 1950s, rules and norms allowed the actors to chart their course with great precision; in the 1980s, greater uncertainty made planning House politics a risky adventure. Finally, political opportunity affects the agenda. When presidents mobilize mass public sentiment, they are able to frighten opponents and to encourage their supporters to seek political victory.

No one will be surprised that the agenda is part of the political calculus in which politicians engage. However, not all will see the deeper implication. Because the stakes of the battle are relatively free to be defined by legislators and because outcomes depend on how the stakes are defined, winning and losing depend on the aggressiveness with which various actors pursue policy at the expense of nominal success. The agenda stage in the political process represents the nexus of important activity; it defines policy. The nominal outcomes are almost uninteresting.

Understanding this, the agenda becomes difficult to study. Because everyone, including the politicians,

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knows the score, the agenda may represent nothing more than a sophisticated version of the outcome. Were this all, agendas would be endogenous to politicians' preferences.

Our evidence, however, suggests that we gain theoretical leverage because exogenous factors affect agenda politics. When selecting these empirical factors, we made a careful attempt to pick ones that were largely independent of any bill's policy mixture. First, while the coalition leaderships choose which bills they back, they do so not as bettors (who back winners for profit) but as full-fledged participants (who expect to shape proposals in return for political support). Second, presidents may go public only on issues that are likely winners. Yet our data indicate that Eisenhower and Reagan chose to go public when they were otherwise likely to lose (Eisenhower on foreign aid, Reagan on program cutbacks). Third, the gross institutional change between the 1950s and the 1980s was very clearly independent of the specific issues that comprise any particular roll call. Finally, all our instances of agenda politics were matters of considerable moment. The president and leaderships could not ignore the issues; choices to support or not support were political decisions, the consequences of which must have been anticipated by all. Hence, their involvement defined—rather than was defined by—the agenda strategy.

In the end, our evidence shows that agendas are strategic because they are grounded in considerations outside the policies at hand and beyond the immediate preferences of the politicians. This we believe to be true and important. We have offered several explanations, including agenda-setters' goal mixtures, uncertainty in the institutional context, and presidential politicization. We do not wish to suggest that these are the only, or the primary, factors involved.[23] Instead, we believe that the evidence establishes the substance of agenda politics as something identifiable, something other than a trivial transformation of options and preferences.

Knowing that the legislative agenda is thus strategic invites us to reconsider the process and the character of structure-induced outcomes. Rather than concentrate solely on institutional arrangements, we need to expand our view to the various agents that govern agenda-setting activity. For it is here that forces exogenous to individual preferences get translated into public policy.

APPENDIX: TESTING THE SENSITIVITY OF THE MEASUREMENT MODEL

Our measurement technique makes two critical assumptions. First, we assume that the main line of House politics can be captured by a single dimension; second, we assume that that dimension is well represented by the liberal–conservative conflict embedded in the ADA ratings. Less heroically, we also assume that the policy preferences of individual members remain (relatively) constant over each eight-year period. These assumptions are, of course, simplifications. We believe that they are reasonable enough. Nevertheless, we offer side evidence that we are on solid ground.

Here, we compare our measurements of the bills' indifference points with those generated by Poole and Rosenthal (1985, 1991).[24] The Poole–Rosenthal measurement strategy makes fewer simplifying assumptions and, in general, is considerably more thoroughly developed than ours. Importantly, the Poole–Rosenthal scoring is based on an analysis of each Congress's entire set of roll calls (not just the Key Votes or the ADA-selected issues). Further, it allows each member's policy ideal point to vary (linearly) over time. Finally, it leaves the dimensionality open for empirical test. (The order of the dimensionality is a matter of continuing discussion; see, for example, Koford 1989 and Poole and Rosenthal 1991 response.) Here, we examine the first (principle) dimension from a two-dimensional solution.

First, and most revealing, note that our ADA-based measure correlates with the Poole–Rosenthal first (policy) dimension at .72 (the Poole–Rosenthal metric is reversed for comparability). Usefully, the overall correlation rises to .84 when three problematic bills for the 1950s are eliminated.[25] The correlation is even higher within political periods (.86 and .89 for the 1950s and 1980s, respectively). The better correspondence for the 1980s is notable because our important inferences are grounded in Reagan-era politics.

Second, the results (which produce the tables in the text) are similar when we compare the Poole–Rosenthal and ADA-based measures of indifference points; that is to say, (1) the indifference points follow the Congress-specific median preferences; (2) liberal agenda-setters produce bills to the right of conservative agenda-setters; (3) liberal leaders sponsor proposals well to the right of the median, conservative leaders produce measures well to the left, and mavericks center around the median; and (4) this leadership–maverick pattern disappears for the 1950s, while it is truly strong for the 1980s. The plebiscitary politics inferences are not statistically replicable in the Poole–Rosenthal data because that series ends in 1985, before Reagan's downfall. (When the later Reagan years are left out of both series, the numerical estimates are similar for both the ADA and the Poole–Rosenthal data, neither producing statistically powerful results.)

Finally, we test the unidimensionality assumption for our Key Votes. Here, we model the aggregate outcome of each vote (both the margin of victory and the probability of victory) on the two indifference point coordinates (one for each of two dimensions) estimated by Poole and Rosenthal. Although it appears that the second dimension may be helpful to account for individual member votes (notably in the 1950s; see Poole and Rosenthal 1991), the aggregate outcomes evince sensitivity only to the indifference point placement on the first dimension. The probabilities that policy concessions on the first and second

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Poole–Rosenthal dimensions *did not* affect the percentage of *yea* votes are $p = .00$ and $p = .98$, respectively, and the probabilities that they *did not* affect the aggregate outcome are $p = .00$ and $p = .94$; that is to say, when agenda-setters manipulated the content of their proposals in order to shape *aggregate* outcomes, they did so most effectively when they concentrated on issues similar to the main liberal–conservative axis.

Notes

An earlier version of this paper was presented at the 1990 annual meeting of the American Political Science Association, San Francisco. The data utilized were made available in part by the Inter-University Consortium for Political and Social Research. The data for voting studies for members of the United States Congress were originally collected by the Congressional Quarterly. We thank Keith Poole and Howard Rosenthal for sharing their data. We are grateful to Jim Weikel for his research assistance. We thank Robert Erikson, Dennis Judd, George Marcus, Gary Miller, Keith Poole, William Riker, David Rohde, Kenneth Shepsle, Terry Sullivan, and Barry Weingast for helpful comments.

1. The idea of a strategic agenda is both simple and subtle. Political scientists use the term *agenda* to describe two distinctive phenomena. The first and more technical use refers to how proposals are sequenced in a legislative body. The formal literature indicates that political actors who control the agenda can often determine collective policy choice. Famously, control over the agenda in the Powell Amendment case allowed conservatives to stall a federal aid to education bill (Riker 1986). The second meaning refers to the substantive content of politics. For example, the agenda of one Congress might be dominated by foreign policy, the next by civil liberties, and the next by budgets. (For excellent work on the substantive agenda, see Kingdon 1984; Shull 1983; Sinclair 1982; and Walker 1977.) Our use of the term *strategic agenda* is closer to the first meaning than to the second. Yet we are concerned with how legislators frame policy choices, rather than the art of manipulating voting sequences. To be sure, most legislative proposals touch on a number of substantive issues. Taking advantage, skillful politicians alter the attractiveness of any proposal by modifying its mixture of policy considerations; that is, they manipulate the strategic agenda (first meaning) by altering the substance of the bills that form the policy agenda (second meaning).

2. We have pruned the initial set of *Congressional Quarterly* bills. First, we have excluded the issues whose definition in the liberal–conservative space is convoluted or over which members' preferences were evidently *not* single-peaked. We examined the votes of each Congress's 10 most liberal and 10 most conservative members and excluded issues on which majorities from the two groups concurred. (This trick eliminates the famous sophisticated agenda game of the Powell Amendment [see Riker 1986], as well as several one-sided issues.) In addition, we have eliminated votes that required super majorities (veto overrides and suspension of rules) because these politics are embedded in a more complicated series of games.

3. We score each member by the average of his or her ADA ratings for the eight-year tenure of the Eisenhower or Reagan administration. We choose to limit this "era-specific" score to the agenda of the relevant administration in order to make the ratings comparable across individuals (see Asher and Weisberg 1978). This multiple-indicator tactic does much to smooth out the ratings that often jump from one year to the next. As a trade-off, we give up an ability to measure genuine (rather than measurement-induced) movement in the members' preferences. We accept this loss because (1) it greatly increases the stability in our "ideal point" estimates and (2) we strongly suspect that members' preferences do not change much from year to year. (For evidence, see Poole and Rosenthal 1991.)

The use of the ADA rating is less arbitrary than it might seem. The rating measures each member's voting agreement with the ADA's preferences on a set of selected roll calls. If one dimension dominates congressional policy voting, then any particular choice of scaling device will prove about as good as any other. We could factor-analyze our Key Votes (using LISREL, MDSCALE, or some other technique) and get another liberal–conservative scale (that would add up liberalism with a marginally different set of item weights). We have chosen to base our analyses in a commonly understood measure (the ADA rating), so that the results might make better intuitive sense. For further discussion and evidence, see the Appendix. Of course, politics and policy are more complicated. We are sympathetic to the richer representations exemplified by Clausen (1973) but here we need to simplify matters to concentrate on the essential question of strategic interaction.

4. We assume that all voting is sincere. (In a unidimensional world, there is no incentive for "sophisticated voting.") Relaxing this assumption, we model voting as though insincere voting were minimal. We have eliminated roll calls that were demonstrably "sophisticated." See n. 2. In their original exposition, Poole and Rosenthal (1985) chose to treat surprise votes as "errors" from proper judgments. The labeling is a matter of semantics. We only wish to note that the "other" factors play the same role here as they do in any empirical estimation equation: they represent unspecified but potentially interesting and important factors. In practice, we minimize the squared errors, assuming a quadratic loss function. Our estimates stem from an initial grid search and then a steepest descent gradient method—all limited to the unidimensional ADA scale. For each Key Vote, we begin with an initial guess, calculate the sum of squared ADA distances from each legislator's ideal point to the selected indifference point (for those voting "incorrectly"), choose another possible indifference point estimate, calculate the "errors," and decide between the previous and current estimates on the basis of a minimum squared error, and so on. In all cases, convergence was achieved fairly easily. Asymptotic standard errors run in the range of about two points on the 0–100 scale.

5. The conservative opponent has no incentive to offer something to the left of the median preference. In fact, the opponent will choose a counterproposal that is just slightly less to the right of the median voter than the liberal proposal is to the left.

6. That the agenda game is sequential is critical. When agenda-setters must anticipate counterstrategies on the part of opponents, the practical alternative to any proposal is *not* the status quo but, instead, a guess about what is likely to occur without the bill. In particular, agenda-setters cannot safely choose proposals that merely beat the status quo; they must expect their opponents to nominate an even more attractive alternative. This judgment is, to be sure, subjective. Fundamentally, agenda politics is rooted in anticipated reactions. We thank Gary Miller for suggesting the importance of this point. The implicit bargaining that characterizes Washington politics are similarly explored in Peterson (1990a, 1990b) and Sullivan (1988a, 1988b, 1990a, 1990b).

7. Here, as later, we treat the intervals in indifference points as linear in the explanatory variables. Obviously, they cannot be absolutely linear for all combinations, since they are bounded 0–100.

8. We introduce this distinction with care. By dividing by *eventual outcome*, we introduce information that was not, strictly speaking, available to the agenda-setters when they developed their strategies. However, on Key Votes at least, it is plausible that the contestants would have vote counts good enough to know whether it made sense to compromise or not. (The choice of 80-vote margins is arbitrary; larger and smaller cutoff points work about as well.)

9. This characterization applies not only to the motives of individual members but also to the rationales motivating collective leaderships.

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10. That legislators often choose to pursue the longer-run game of symbolic politics over the possible gains offered by current compromise is underappreciated. As Gilmour (1990) indicates, such a political strategy has often produced policy outcomes (or nonoutcomes) distinct from those one might expect, given a short-run perspective. Symbolic politics may be more prevalent than is often thought.

11. The downside is the median preference position, because we assume that all proposals are part of a sequence (see n. 6). Future proposals converging to the median preference position will always beat the status quo.

12. We treat the agenda-setter as part of a protocoalition, not as an individual with personal preferences. In House politics, on major issues, members do considerable politicking; they form coalitions before an item reaches the floor. The nominal agenda-setter may be a mere figurehead for the real strategists. Thus, we limit our identification of the actor to a liberal–conservative tendency. Occasionally, things get a little murky. We have excluded from our analysis a handful of items in which the impetus behind a bill was bipartisan (the president joining with both party leaderships), the coalition of movers and supporters did not fit a liberal–conservative coalition battle, or the agenda-setter's motives appear potentially insincere.

13. For example, consider the liberal agenda-setter faced with a conservative proposal five points to the right of the median preference. The policy shaper may counter with a moderate liberal bill (say, three points to the left of the median) and hope that the sequence stops before converging on the median. The powerseeker, however, might counter with a marginally conservative proposal (say, three points to the right) to ensure a win. All liberals and a few moderate conservatives will prefer the new proposal over the old, and the conservative agenda strategist will be stymied. Thus, while shrewdly gaining nominal success, this liberal agenda-setter will produce a less liberal policy than would otherwise be possible. The political game reverts to claiming victory in the postgame interviews.

14. Similar numbers appear when we model the indifference points with the median voter in the right-hand side of the equation, adding 3% to the fit. We have chosen to present the model as deviations from the median voter, so that the parameter estimates will show the theoretically important distinctions. The signs of the coefficients reflect the directional component of the ADA score: *positive scores are more liberal, negative scores are more conservative.*

15. Here, the liberal and conservative leaderships are respectively indicated by a unanimity of support from (1) the Democratic leader and whip or (2) the president, Republican leader, and whip. (Abstention "breaks" the leadership.) The leadership variables are constructed to include only those cases in which a liberal or a conservative initiates an action with the support of the respective leadership. (Notice that the "liberal" leadership is equivalent to the formal House leadership for all years except 1953–54.)

16. Shepsle (1979) coined the phrase; for theoretically important extensions, see Denzau and Mackay 1981, 1983; Krehbiel 1987; and Shepsle and Weingast 1981, 1987. Examining cross-institutional structure, Hammond and Miller (1987) produce "constitutionally induced" stability in equilibria. The rise of uncertainty has been addressed in two ways: (1) legislators are uncertain about the other legislators' preferences (Denzau and Mackay 1983; Enelow 1984; and Enelow and Hinich 1983a, 1983b); and (2) the relationship between bills (policy choices) and outcomes (policy consequences) is uncertain (Krehbiel 1988). In addition, the effect of presidential strategic behavior on stable equilibria is addressed in regard to appropriations decisions by Kiewiet and McCubbins (1985, 1988).

17. The 1980s leadership responded to increased uncertainty by increasing the use of special and restricted rules. The extent to which this tactic has reduced uncertainty is not yet clear. Bach and Smith (1988) suggest that the new procedures have had the effect of reducing uncertainty in process but not in policy, that is, while the number and types of amendments have been restricted, the ideological content of them has not.

18. This result is less tidy than it seems. Note that floor voting was less clearly determined by matters of ideology in the 1950s than in the 1980s. The median R-squared between a member's ADA score and vote on the Key Votes was .33 during the 1950s and .54 during the 1980s. Thus, in policy terms, the 1980s were a period of greater certainty than the 1950s. However, recall that the nonideological factors influencing voting include the sorts of informal leverage—the side payments—associated with internal control of the House. By all historical accounts, the 1950s party leadership controlled and exercised this nonpolicy leverage over individual members' choices: "go along, get along." We get an initial resolution by looking at "ideological" and "nonideological" issues for both the 1950s and the 1980s. If all is a matter of ideological structure, then the agenda-setter model should be seen to work better for the more ideological issues. On the other hand, if the theoretical perspective associated with agenda uncertainty predominates, then we should see something more subtle. In the 1980s, what was nonideological was uncertain; thus, the model should produce a stronger result for the nonideological issues. Crucially, though, the 1950s leadership's greater control over the nonideological factors should produce a weak agenda-setter result for the nonideological issues. And this is just what the data show. The following table shows the signed difference between the conservative and liberal leadership agendas for the 1950s and 1980s. (High and low ideological content refer to scores above and below, respectively, the overall median R-squared between individual ADA score and roll call vote [.40]; −.35 is a wrong sign; for the remaining data, $p < .05$; Ns are in parentheses.)

	1950s	1980s
High ideological content	6.56 (22)	6.73 (68)
Low ideological content	−.35 (32)	10.82 (36)

Notice that things align nicely for both types of issues in the 1980s, with the conservatives producing agendas (indifference points) well to the left of the liberal agendas. Contrary to the "ideological" explanation, the difference is larger for issues of lower ideological content. Notice especially, however, that the pattern is reversed in the 1950s. Because nonpolicy matters were largely under the control of Rayburn and his barons, the leaderships were able to pursue policy success (with a minimal safety margin) most clearly on the issues whose policy content was least ideological. (This result is particularly powerful because it is produced by a nonlinearity that falls directly out of theory.) Uncertainty is what matters here.

19. We measure presidential prestige by using the familiar Gallup approval polls. This is the common presidential approval measure that has dominated studies on presidential success or influence with Congress (Bond and Fleisher 1980, 1984; Edwards 1976, 1980, 1989). The data are taken from Edwards's (1990) compilation of the series and aggregated to quarters. Further, because we have data that span two different presidencies, from two different political eras, we subtract each president's mean score from the relevant reading. (Eisenhower was, on the whole, much more popular than Reagan.)

As suggested by Kernell (1986), we measure *going public* in two forms. We distinguish between major and minor addresses, with major addresses being televised talks and addresses to a joint session of Congress and minor addresses being those directed toward a specific constituency. For all of Eisenhower and for the first five years of the Reagan administration, we took Kernell's (1986) coding for the major public

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addresses. We obtained a measure of major addresses for the remaining three years of the Reagan administration and minor addresses for both presidencies from the Public Affairs Information Service Index. After some experimentation, we settled on a variable that counts both major and minor address (if they occurred in the half-year preceding the key vote). Further, we decided to indicate going public in simple dichotomies, that is, whether he went public or not (as a conservative or a liberal). The additional information, assessing the amount of speechmaking, contributed little to our explanation. Our simplification eases the interpretation of the empirical results.

20. For the entire data set, the relevant parameter estimates are as follows (cf. Table 3).

Variable	Parameter	SE	p
Approval	.13	.13	.32
Go public (conservative)	.18	3.10	.95
Go public (liberal)	2.91	4.99	.56

with the remainder of the table resembling Table 3.

21. Here, going public and approval (the deviation from the president's mean) are multiplied by one for agendas offered by liberals and by minus-one for agendas offered by conservatives. Thus, a negative coefficient (as seen in Table 5) indicates a *concession* on the part of the agenda-setter.

22. We do not wish to make too much of statistical inference here. Of our 158 Key Votes, only 20 were accompanied by the president's going public. In effect, our estimates' magnitude and precision depend on those 20 cases. Given the paucity of data, we suspect these "significance" tests to belie stronger relationships than we can prove. Further, these are two-tailed tests. Obviously, a more powerful view asserts a one-tailed test (given our theory) and generates p-values of one half those in the text.

23. Our empirical model is very simple. In essence, it includes the Congress median ADA score, the presence or absence of the House leaderships, and (marginally) the popularity and speechifying of the president. We do not expect to account for the full subtleties in agenda strategy with such a simple model. Instead, we only wish to establish the strategic character of those agendas qualitatively. Nevertheless, our models do account for about one-fifth of the variance in indifference points. If we wish to engage in the variance-accounted-for exercise, the full model implied in Table 4 (which takes into account era-specific differences) accounts for 28% of the total variance. Further, for this exercise, we may exclude relatively one-sided contests (bills whose margins of victory or defeat exceeded 80 votes) on the grounds that our model cannot explain strategic behavior beyond establishing a substantial "safety margin." Then the simple model "accounts for" 40% of the variance and produces a root mean squared error of 5.71 ADA points. This exercise merely establishes the plausibility of our simplification; it does not propose our model as a complete substantive explanation.

24. We should like to express our thanks to Keith Poole and Howard Rosenthal for kindly making their data available to us.

25. The three bills include two foreign aid bills (1957 and 1960) intended to enhance appropriations beyond the committee report. Rather than relying on the Democratic leadership for political leverage (as they typically did in such cases), the administration pushed its case against the united Democrats. The third is the civil rights bill of 1957, which (as ever) split dominant coalitional bases. Of our Key Votes, only these are estimated (by Poole and Rosenthal) to have indifference points *outside* the cloud of legislators on the first dimension; thus, they are poorly replicated in one dimension (the prediction is a unanimous vote)—hence the trouble.

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ERIC VOEGELIN'S THEORY OF CONSCIOUSNESS

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The self stands in the way of understanding and appreciating consciousness. The self is a reflexive, asymmetrical bit of consciousness that displaces the whole of consciousness and the open nature of consciousness. Such displacements of consciousness are expressions of gnosticism. Some political movements (e.g., fascism) are expressions of such restrictions of the horizon of consciousness. To understand and appreciate consciousness requires a degree of selflessness on the part of the inquirer. Philosophy and myth are modes of discourse that embody and cultivate the selflessness necessary for participating in, appreciating, and understanding consciousness. Such philosophizing enhances the possibility of more inclusive joint political action.

Eric Voegelin is explicit about the political significance of a theory of consciousness. He says that his "analysis of the movements of Communism, Fascism, National Socialism, and racism, of constitutionalism, liberalism, and authoritarianism had made it clear beyond a doubt that the center of a philosophy of politics had to be a theory of consciousness" (1977, 3).[1]

I shall elucidate Voegelin's theory of consciousness and show how it is central to a philosophy of politics. We begin to see the relationship between consciousness and politics by noting that the point of political action is not only to accomplish particular purposes but also "to speak solemnly in everyone's name, in the name of society, about what it holds dear" (Nozick 1989, 289). This is so because joint political action constitutes a relational tie; by political action we "express and instantiate ties of concern to our fellows" (p. 288). The ties of concern and the values of the community go together; without the relational ties, it would not matter to us whether or not the others were included in regard to what we held dear. Voegelin's theory of consciousness helps to explain the nature of these relational ties of concern at the heart of politics. His theory especially helps to explain their absence.

The thrust of my argument is that by its very nature the *self* cannot participate in, and appreciate, the reality of the whole of consciousness. One form that participation and appreciation of the reality of the whole of consciousness takes is philosophy. Voegelin describes the participation that is philosophy as the "compelling experience of eternal being in time," an "experience of transcendence" (1964, 117). Thus, the experience of philosophy *is* the transcendence of self, not merely a means to such transcendence. This existential, or experiential, quality of Voegelin's meaning is absolutely essential and is also reflected in his pronouncement on the shortcoming of histories of ideas, namely, that they do not adequately take into account experience and its symbols (1989, 63). (It is also true of the self that it does not adequately enter into, and account for, experience and its symbols.) This experiential dimension points to the significance for philosophy and consciousness of "anamnetic experiences," in which transcendence of self both occurs and can be further cultivated. Transcendence, however, does not remove the "tension in existential reality pointing toward an order beyond itself" (Gebhardt 1987, 117). Political movements such as fascism and racism are extreme forms of "selfness"—"restrictions of the horizon of consciousness" (Voegelin 1977, 4). Moreover, philosophy as participation in consciousness (the experience of eternal being in time, or transcendence) enhances both knowledge of the political significance of the self and the possibility (through transcendence of the self) of more inclusive joint political action.

SELF AND CONSCIOUSNESS

The first and absolutely critical step toward understanding consciousness is to distinguish consciousness from the I, or self (hereafter used interchangeably). According to Voegelin (1978):

> It is doubtful whether consciousness has the form of the I, or whether the I is not rather a phenomenon in the consciousness. (p. 19)
>
> Consciousness is not constituted as a stream within the I. (p. 36)
>
> No "human" in his reflection on consciousness and its nature can make consciousness an "object" over against him. . . . (p. 33)

To make an object over against entails the formation of the self; the self is the subject that stands over against, that separates itself from the objects. The presence of the self, therefore, constitutes a denial by the self of the whole of consciousness. From a perspective outside or beyond the self, it is possible to see that such denial does not result in the absence of consciousness, since consciousness is ever present as the *context* within which the I exists. But for the self, consciousness as a whole is not present. Voegelin calls the self the transcendental I and says that "the creation of the transcendental I . . . implied the destruction of the cosmic whole in the subjectivity of the egological sphere" (1978, xi).

Voegelin's attention is on consciousness, rather than the self; the nature of the self emerges in his

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work only indirectly as he clarifies the nature of consciousness. In contrast, an analysis that focuses directly on the self appears in the recent work of Robert Nozick (1989). We turn immediately to Nozick because seeing the self sharply makes it easier to understand Voegelin's analysis of consciousness. The essential point that emerges is that with the formation of the self, one is kept from understanding, participating in, and appreciating the whole of consciousness.

For Nozick, as well as for Voegelin, self and consciousness are connected. Nozick maintains that the self is "a bit of consciousness." Bits of consciousness may be considered experiences, thoughts, and so on (1989, 144). Each such bit is a case of *figuration* (Barfield 1965, 18). Figuration is what is added by the percipient to pure sensation to form a representation of what had been unrepresented (i.e., what had been formless to the percipient). Two steps are involved in figuration: (1) the sense organs are related to the unrepresented so as to elicit sensation and (2) the sensations are constructed by the percipient's mind into a representation (p. 20). Some figurations are figurations of other figurations. In other words, some bits of consciousness include, and are about, other bits. An example is a memory of an event: the event is a bit that comes to be included in the memory bit of consciousness.

The self is one of these bits, but it is different from the others in that "this bit is an awareness of many of the other bits of experience and thought, *plus* an awareness of itself, a reflexive self-awareness" (Nozick 1989, 145). Reflexive self-awareness entails awareness of an awareness that is unique: the awareness of "I," "me," or "myself"—not *any* awareness. Such awareness is exclusionary; it draws a boundary that includes some bits of consciousness and excludes others. Certain of the enclosed bits are called "me"; somehow, those bits are separated from others, then identified with (appropriated, acquired).[2]

The self entails asymmetrical knowing (Nozick 1989, 145). The bit that is the self knows the other bits; but as a self, it is uninterested in being known by other bits that are less important than it is. As the center of awareness (although unconsciously so), the self is superior to other bits. The self orders them, determining how important each is, which to ignore, which to repress, and so on. Every self might be said to be the sovereign among bits of consciousness; each self is like a miniature modern state within the vast field of bits of consciousness. Its sovereinty lies in the power to demarcate that part of all consciousness it calls itself. In this regard, the self is unique; only it has this particular demarcation, this particular set of bits of consciousness ordered in its particular way (p. 146). In sum, the sovereignty of the self lies in its self-interested control of awareness.

The self is not necessarily stagnant; it may continue acquiring, ordering, and identifying itself with new sets of bits of consciousness. It may also stop acquiring new bits and may lose what it has acquired. Yet no matter what the size of its "territory," the self does not give up its sovereignty. In other words, its asymmetrical relationship to its objects does not change.

The self does not allow itself to become equal to all the other bits of consciousness. If it did, it would disappear; and consciousness as the whole of consciousness could be present. The presence of the whole of consciousness, however, cannot be known in a conventional way of knowing; for conventional knowing is the knowing of the self. Voegelin points out that "it is the function of human consciousness *not* to flow but, rather, to constitute the spaceless and timeless world of meaning, sense, and the soul's order" (1978, 16). The knowing of the self is not the kind of knowing that can know a spaceless and timeless world of meaning.

One can see the asymmetrical nature of the self more sharply by positing a context of symmetry or *equality of bits*. Assume that all awarenesses (bits of consciousness) are equal. Then no one of them would unilaterally consider its "knowledge" of reality to be superior to any of the others' "knowledge" of reality. Each "knowledge" would be equal in the sense that no one bit would be inclined to impose itself on any other. The whole of consciousness entails such equality of bits; by its inherent asymmetry (inequality), the self disrupts that equality. As a bit of consciousness, in contrast to all of consciousness, the self orients itself by its own perspective. Thus, by its very own existence, the self does not and cannot realize that it is not open to grasping and appreciating its actual situation—that is, its context, consciousness. (One interpretation of Genesis 3 is that eating from the tree of knowledge represents the emergence of the self and constitutes the source of human alienation.) In short, the self, because of its asymmetrical nature, displaces consciousness. This displacement is one form of what Voegelin means by *gnosticism*.

The asymmetrical knowing constitutive of the self is, in theological terms, a form of *idolatry*, that is, the "identifying [of] reality with the picture of representation of it framed by the conceptualizing imagination" (Cochrane 1940, 244). (The conceptualizing imagination appears in the form of the self.) Idolatry is also described as "the effective tendency to abstract the sense-content from the whole representation and seek that for its own sake, transmuting the admired image into a desired object" (Barfield 1965, 111). In short, idolatry is a case of the fallacy of misplaced concreteness, of treating "what is but an abstract aspect of some larger whole as though it were the whole itself, or at any rate something concrete" (Ogden 1982, 29). Inasmuch as this is precisely what the self does, it seems accurate to describe the self as a form of idolatry.

The self has a tendency to project (usually unawares) its order of bits of consciousness on others as if its reality were *the* whole truth; in effect, it wants its reality to be the only reality. The self's identity is idolatrous in the sense that it rejects the whole of consciousness for its particular version. It does not say to itself, "Well, I know that the reality that I am

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aware of is a distortion of reality itself, taking into account only a limited, and therefore biased, view." Rather, the self identifies unawares with its organization of bits of consciousness. It rejects the equality of bits in spite of the fact that it will itself likely change as it reorders itself to take into account new bits of consciousness. Indeed, it may relate to each newly ordered set of bits of consciousness by identifying itself unawares with that set. It does this time and time again.

The self is busy protecting itself against the ongoing attempts of other selves to displace it (Hobbes 1958, 86–88). In fact, it is so busy protecting its identity against the threat of other selves that it cannot give itself up. This is understandable; for it maintains its identity and assymetry by identifying with bits of consciousness, or content (as opposed to appreciating the whole of consciousness, or context). If it were to understand, accept, and appreciate consciousness (the context that is realized, without the self, to be timeless and spaceless), it would so open itself to the equality of other sets of consciousness as to lose its power of asymmetry: it would lose selfhood itself. If it did that—gave itself up—it would experience transcendence.

TRANSCENDENCE

The difficulties encountered in regard to thinking about consciousness are similar to the difficulties experienced in thinking about God. Robert Sokolowski, discussing Saint Anselm's argument for the existence of God, says that "God must be thought to exist; for if he were seen to exist only in the mind, he would not be appreciated as that than which nothing greater can be thought" (Sokolowski 1982, 8). "God must be thought to exist" does not mean that the thought "God exists" is necessary if God is to exist. Rather, it means that the presence of God is the whole of reality, which includes more than all thoughts existing in all of human consciousness—a reality much more inclusive than the thought "God must be thought to exist."

"That than which nothing greater can be thought" points toward God, or absolute, ultimate, all-inclusive, reality—that is, consciousness. Consciousness, the whole of reality, may be considered a context or, more accurately, *the* context (the context of all contexts). Some non-Christian theological traditions speak of this context in strikingly similar language. In the Hindu scripture we read, "That which is not thought by the mind but by which the mind thinks—know that as the Absolute" (Kena Upanishad 1.6).

Voegelin describes consciousness "as the experience of a finite process between birth and death" (1978, x). He also speaks of "tensions between human consciousness and transcendence" and says that "the capacity of transcendence is a fundamental character of consciousness" (p. xi). Most importantly, Voegelin speaks about the radicalism and breadth of philosophical reflection. He says that the radicalism of philosophizing must be gauged by "the experiences that impel toward reflection and do so because they have excited consciousness to the 'awe' of existence" (1943, 36).

Experiences that inspire the awe of existence (e.g., anamnetic experiences) are in sharp contrast to those of the self. The self is comprised of what the mind has thought. The self is a bit of consciousness always having content and identifying with content, which the self as the mind has provided. Just as the Absolute is never the content of what has been thought (but rather, is that by which the content appears), neither is consciousness ever exhausted by what has been thought (i.e., by content). Consciousness and philosophy converge in open wonder—in the awe of existence that sees beyond bits of consciousness—and opens the soul to further experiences of wonder and awe (1978, 36). Thus, in comparison to the life of philosophizing, the life of the self has a dead quality. The self seems to exist in the already-existing past (in the self's mind), while the *soul* lives in the wonderous opening of the present, of the ground of being (the "compelling experience of eternal being in time").

MYTHOLOGY AND POLITICAL SCIENCE

The importance of the experience of the awe of existence must not be underestimated. Indeed, Voegelin says that "the decisive event in the establishment of *politike episteme* was the specifically philosophical realization that the levels of being discernible within the world are surmounted by a transcendent source of being and its order" (1968, 18). The Platonic–Aristotelian analysis operates "on the assumption that there is an order of being accessible to a science beyond opinion" (p. 17). This science beyond opinion rests on experience beyond the self. In other words, the self is a barrier to the infinite experience of awe and the transcendent order of being. No science of politics is therefore possible when the self in its existence shuts off the opening of the soul to the transcendent order of being.

The words *specifically philosophical realization* imply that philosophy *is* the meditative state of being in which the self drops away into transcendence. Philosophy is not the means to transcendence of the self, it is such transcendence. Certain difficulties arise in connection with this. Voegelin refers to "the experience of meditation, at the climax of which consciousness apprehends the contents of the world nonobjectively" (1978, xi). This means that the "knowing," or apprehending, of the contents of the world by meditative consciousness is not the knowing of the self; for the knowing of the self is not nonobjective. The knowing of the self entails a subject over against objects, which, because of the asymmetrical nature of the self (a subject), forecloses the possibility of nonobjective, transcendental, meditative knowing. This point is applicable to anyone reading, writing, or

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thinking about consciousness as Voegelin presents it. In other words, the meaning of Voegelin is not available to us insofar as we are only selves.

It makes sense, therefore, to turn to modes of expression in which it is more difficult for the self to operate and to modes that are not expressions of the existence of the self (although the receiving self is not likely to recognize these for what they are). This is what Voegelin does both in his essay "Anamnetic Experiments" (1978) and in his discussions on the nature of myth. (One could say, moreover, that it is possible to see in Voegelin's philosophizing the remarkable open seeing and expression of selflessness.) Anamnetic experiments are recollections of "experiences that have opened sources of excitement, from which issue the urge to further philosophical reflection" (1978, 37). Such experiences are those that excite consciousness "to the 'awe' of existence" (p. 36).

Voegelin invites us to participate in these experiences, thereby cultivating for ourselves the fundamental insight upon which *politike episteme* is based—namely, transcendence of self, consciousness of the awe of existence. Voegelin writes of himself: "A philosopher, it appeared, had to engage in an anamnetic exploration of his own consciousness in order to discover its constitution by his own experiences of reality, if he wanted to be critically aware of what he was doing" (1977, 12–13). Conceptual analysis or theorizing about consciousness is inadequate; one must explore "concretely in the constitution of the responding and verifying consciousness. And that concrete consciousness was my own" (p. 12).

Voegelin points out that Plato may have used myth "because he felt that the myth is a more precise instrument for communicating the psychic excitement of the experience of transcendence, more precise than speculation, even though it is an instrument which not everybody can handle safely" (1978, 22). "A mythical symbol," Voegelin says, "is a finite symbol supposed to provide 'transparence' for a transfinite process" (p. 21). Myths are necessary because "processes transcending consciousness are not experienceable from within" (ibid.). We must use symbols that have developed on occasions of finite experience to evoke the transcendent, the opening to further opening, the transfinite awe of existence. Myths, in short, have arisen as a means of expressing the tension within consciousness between the finite and the infinite. Mythical symbols, while likely to be finitized by the self that misunderstands the nature of such symbolic forms, are actually intended to open the soul to the transcendent (the experience of which goes beyond what the self as self will allow).

"To speak of a process as infinite," writes Voegelin, "is tantamount to saying that we have no experience of it 'as a whole' " (1978, 21). Opening ourselves to the infinite whole is precisely what the existence of the self rejects and precisely what myth and philosophy invite. The whole and the self are at odds, of course, for the whole and asymmetrical knowing are incompatible.

Mircea Eliade points out that all myths are in a sense *creation* myths because they evoke the sacred "time" that existed at the beginnings of everything. Even the telling of a myth may be an attempt to evoke the timeless and spaceless beginning (that beginning even before time and space). Voegelin says that "we have no experience whatsoever of a time in which something might begin—for the only time of which we do have experience is the inner experience of the illuminated dimension of consciousness, the process that drops away, at both ends, into inexperienceable darkness" (1978, 21). The sacred, which is creation, is beyond the experience of consciousness; to suggest otherwise would be to *subjectify* (make subject to the self) that which cannot be made subject. That which is subjectified (and thus deformed) is no longer awesome. The very purpose of myth, then, to evoke the opening of the soul to creation, is subverted by the subjectivity of self.

Voegelin explicitly discusses the creation myth of Genesis 1 (1987, 19–20). He points out that its authors faced the task of finding language symbols that would express the experience and structure of the infinite quality of consciousness, ("as an event of participation between partners in the community of being" [1987, 15]), as opposed to consciousness in its finite, "thing" form of selfness. Genesis 1 gives expression to humans participating not as selves but as partners of the wondrous "comprehending reality" (p. 16).

In spatial terms, Voegelin locates this participation, this reality of the "luminosity of consciousness" " 'between' [selfless] human consciousness in bodily existence and reality intended in its [self-ful] mode of thingness" (1987, 16). The "in-between" (Plato's *metaxy*) is meant to show that consciousness in its infinity does not occur in the presence of the self, but rather "in the In-Between of the divine and the human" (1989, 73). The reality of consciousness as infinity is not outside human consciousness (ibid.); for if it were, the self as subject over against object would again have raised its head. Not being outside, it is in the in-between, that is, the context that consciousness participates in (the community of being). Myths give expression to this context and participation in it.[3]

This points to the noumenal, rather than the phenomenal, nature of consciousness (1978, 32). As the thing-in-itself,[4] the noumenal is inaccessible to the mind. The noumenal takes us back to the discussion of the nature of context and God as exemplified by the Kena Upanishad. Consciousness is noumenal. Like myth, it is revelatory. It enables us to live more abundantly, beyond the limits of ego and mind.

GNOSTICISM

Voegelin emphasizes the inescapable nonobjectifiable nature of consciousness in the actual fullness of reality. It is the self that resists the whole of reality. The self by its very nature is and must be an act of "deformation," or deception; its self-deception is

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"believing the reality of which it is a part to be an object external to itself that can be mastered by bringing it into the form of a system" (1977, 4). This goes to the very heart of what Voegelin means by gnosticism. The gnostic believes that evil, or the drawbacks of human life, "can be attributed to the fact that the world is intrinsically poorly organized" (1968, 86). With knowledge (gnosis) of the method of altering being, the organization of the world can be altered; salvation from the evil of the world is possible (p. 87).

However, "the specifically human mode of participation in reality" is consciousness (1977, 4). Human consciousness is "an event in the reality of which as a part it partakes" (1978, 4). Thus, human consciousness cannot separate itself out of reality and then see either itself or the rest of reality as objects. Human consciousness remains an event in a context that is both finite and infinite. To make of itself an object over against reality is to not see the whole of its situation; and to not be able to see the whole of its situation is an essential aspect of its situation—the essential infinite quality of consciousness. In believing that the reality in which it exists can be objectified and systematized, the gnostic engages in idolatry, treating "what is but an abstract aspect of some larger whole as though it were the whole itself, or at any rate something concrete" (Ogden 1982, 29).

Moreover, gnosticism is a rejection of philosophy itself, for thinking that such systematic knowledge exists undermines that authentic transcending sense of the awe of existence that constitutes the opening that keeps on opening. Voegelin writes reverently about the "responsive openness to the appeal of reality" and openness to the ceaseless action of consciousness "expanding, ordering, articulating, and correcting itself" (1977, 4). In sum, gnosticism is a denial of the whole of human consciousness, a denial of "the specifically human mode of participation in reality" (ibid.).

CONCLUSION

The essential point is that from the perspective of the self, consciousness in its fullness does not exist. For the self (not the whole person or necessarily the individual but the self), there is no context in which the self partakes, no context that includes the transcending infinite as well as the finite. The self is a case of what Voegelin calls a restriction of the horizon of consciousness (1977, 4).

The importance of this restriction of the horizon of consciousness is that it provides a link between consciousness and the political movements that Voegelin was intent upon understanding: communism, fascism, national socialism, and racism, and also constitutionalism, liberalism, and authoritarianism (1977, 3). These movements are to varying degrees expressions of the self, expressions of the restriction of the horizon of consciousness. Thus, they cannot be understood fully as ideas or as ideology; they must be understood as human actions flowing out of selfness, the denial of consciousness in its fullness.

These movements cannot be fully understood, therefore, by selves—those who as subjects would objectify these movements. Investigators of these movements, in addition to conventional historical and comparative study, must examine their own selves, conduct the anamnestic experiments and philosophical meditations to comes to terms with their own relationships to consciousness, their own modes of participating in reality (see Tulka 1987).[5] Only after having conducted such experiments is the investigator in a position to understand these political movements in their fullness.

A theory of consciousness is not just essential for such understanding of political phenomena. It is unavoidable in the sense that "all philosophizing about consciousness is an event in the consciousness of philosophizing and presupposes this consciousness with its structures" (1978, 33). There is no pure consciousness of philosophizing; such consciousness is that of a human being still within the context that is transcendent, as well as finite. That context constitutes consciousness, which is preferable, Voegelin says, "to a reality cognitively constituted by a transcendental ego" (1977, 12).

Plato makes this point in the *Gorgias* via the symbolism of the Last Judgment (1959, 111). The Judgment is, in effect, the evocation of consciousness or selflessness; for it imagines the observation of one's entire life by an omniscient judge rather than by a self. Consciousness includes the possibility through experience and meditation (especially through myth and philosophy) of entering such a state. The nature of reality (consciousness) is itself the openness of selflessness. There is no place to go with such opening; such opening does not lead anywhere but to further opening, to the awe that kindles the philosophic life, a life of wonder, not a life of answers (except as those "answers" cultivate openness). We may say that such openness is consciousness realizing itself, the space of abeyance of the self of human beings.

What is written here or anywhere by anyone seems subject to the context of consciousness and its structures. This leaves us where we should be—wondering. The kind of wondering in which Voegelin leaves us is the kind that could not have been a party to the destructive mass movements of communism, fascism, and so on. Indeed, the experience of the other and the experience of the self as self are mutually exclusive. "The problem," Voegelin writes, "of the Thou seems to me to resemble that of all other classes of transcendence" (1978, 23). Consciousness of the experience of the other is a "given of experience from which one may start out but behind which one may not retreat. The capacity of transcendence is a fundamental character of consciousness just as much as is illumination; it is given" (ibid.). Consciousness is given, while the self is an attempt to displace consciousness, transcendence, the other, and philosophy.

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Is Voegelin pointing to the possibility of a political community based on the opening of the soul to the transcendent source of order? Such a question perhaps misses a more fundamental point. Without the transcending experience of the awe of existence, one is likely to see politics *only* as the expression of the reality of selfness. (Voegelin accuses Hobbes of this [1952, 180]). Such an understanding is flawed. It is not possible fully to grasp the nature of the self from within selfness. Reality includes both self and consciousness where consciousness is understood as the reality that includes more than self. A theory of politics based exclusively on the experience of self, consequently is incomplete and therefore wanting. Inasmuch as the whole of political life includes and reflects consciousness in its finite and infinite character, one must knowingly experience consciousness in its full range of the finite and infinite to be able to speak authentically on the nature of politics. A theory of consciousness equal to the task of being human and conducting political analysis entails not only ideas about consciousness but experiments whereby the investigators examines their own selves and come to appreciate consciousness in its infinity and finiteness.

Finally, as we noted at the outset, the point of political action is not only to accomplish particular common purposes but also "to speak solemnly in everyone's name, in the name of society, about what it holds dear" (Nozick 1989, 289).

Participation in consciousness (selflessness as "participation between partners in the community of being" [Voegelin 1987, 15]) entails profound recognition of the other. Such participation enhances the possibility of more inclusive joint political action. Moreover, such participation is acutely aware of the persevering nature of the self and its political significance. Transcendence (philosophy) results in more of both reality and possibility.

Notes

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1. For secondary sources on Voegelin's theory of consciousness see Cooper 1986; Germino 1982; Havard 1971; Keulman 1990; Porter 1975; Sandoz 1981; Webb 1981; Webb 1988. None of these equally emphasizes the character of the self as an obstacle to the appreciation of consciousness in its fullness.

2. It goes beyond my scope here to reflect upon both the nature of property in connection with the self and liberalism as a social and political expression of selfness.

3. Ellis Sandoz emphasizes (1) that Voegelin's philosophical inquiry is "one of the modest remedies against the disorders of the time" (1987, 4; read *of the self*); (2) that the problems of transcendence were the decisive problems of Voegelin's philosophy, citing his words, "Philosophizing seems to me to be in essence the interpretation of experiences of transcendence" (p. 5).

4. This is not to be confused with the thing over against which the self is a subject.

5. Consider the following exercise: "Note the kind of mental events that come up in the mind and their relationship: images and feelings, intentions, thoughts, judgments, etc. Which elements are dominant and which supportive? How do states such as tension, conflict, boredom, calm, clarity, or appreciation arise? Keep a journal in which you record your discoveries: an annotated catalogue or inventory of the contents of your mind" (Tulka 1987, 15). The design of this exercise is to enable one to be free from unconsciously identifying with the content of the mind, that is, free from the self. The exercise thereby opens up the possibility of participating in the whole of consciousness, rather than in the deformed reality of the self.

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POLITICS AND THE PROBLEM OF TECHNOLOGY: AN ESSAY ON HEIDEGGER AND THE TRADITION OF POLITICAL PHILOSOPHY

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I examine Heidegger's postmodern interpretation of technology, with an eye to exposing its weaknesses. I do this by showing that his view entails an understanding of the tradition of political philosophy that cannot do justice to that tradition's own understanding of the character of technology. I consider first Plato and Aristotle and then Hobbes and Locke in order to suggest that Heidegger's view that modern politics are stamped *by technological metaphysics can be challenged on two related grounds: (1) it assumes incorrectly that the tradition is dominated by dogmatic metaphysics; (2) it prevents us from seeing how some in the tradition both understood and doubted the possibility of a technological stamp. I then suggest an alternative view of technology that might account better for the character of technology and political life.*

Recent thinking about technology and its implications for politics has taken a postmodern turn. In its modern formulation, the danger of technology is the possibility that in the scientific age we will succumb to the consequences of our most impressive intellectual achievements. First, we were poised on the brink of nuclear destruction. Now, as that threat fades, we and the world face new dangers, this time springing from technology used with the best of intentions. Disaster looms because technology enhances our powers of control and because modern science, the theoretical ground of technology, understands human nature to be as manipulable as matter in motion. Under the spell of these forces we risk self-inflicted deformation and brutalization, all in the name of human comfort, freedom from pain, and variety of choice. From this modern point of view technology compels us to remember our essential humanity lest we fail to control the technical means that threaten fundamental values (e.g., Jonas 1984; Kass 1985). The modern age is not just the age of science; it is the age of the potentially beneficent tension between science and humanism. If we understand how science and humanism contend within the progress of reason, it will be possible to exercise responsible political choice today.

The postmodern view is perhaps best expressed by Heidegger's comment that "the essence of technology is by no means anything technological" (Heidegger 1977, 4; 1967, vol. 1, 5). Technology is not the sum of machines and techniques that we must learn to master and use as a neutral set of means lest we forsake our humanity. Technology is not even something we need to "do" anything about. Rather, it is just what we have now become. And although it opens our eyes, it does not compel us to seek out a reinvigorated humanism. On the contrary, technology discloses to us that humanism is the proximate source of technology. Technology is indeed a fateful danger; but it shows us that within the tradition of rationalism, the essences of technology and humanism are the same. As the result of that tradition, our world is now stamped by technology. But the problem of technology has nothing to do with recovering humane standards for political (and technical) choice. That problem is less a challenge to be met and overcome than a sign that we are on the verge of an age in which all of our categories of political choice and evaluation have become exhausted. Whatever it might be that technology compels us to do (especially as regards politics), it will not be to recur to the contending elements of modern rationalism.

I wish to suggest that Heidegger's view of technology is untrustworthy and that an indication of its weakness is that it misinterprets the theoretical tradition that it claims *stamps* the modern era. His view obscures the extent to which the most important thinkers in that tradition distinguished between political life and the activities of thought, or the power of any tradition. Heidegger's view obscures the extent to which these thinkers were themselves aware of the phenomenon of the technological stamp and knew better than Heidegger himself the limited power of any such stamp to form practical life. Moreover, they understood how the stamp is an ineradicable, natural fact of productive life. I argue that we can learn more about technology from the thinkers of the tradition than from Heidegger, and then suggest some lines along which we might think to recover and develop an alternative view of the problem of technology that we face.

NIHILISM AND BEING

For Heidegger, modern science and technology are rooted conjointly in the metaphysical worldview. According to this view, the world is conceived of as a spatial whole made up of three parts. These three parts are the demonstrably knowable and eternal *ground* (objective laws of matter) of every particular entity; all the particular *entities*; and the human *subject* who discovers the objective ground and lives among the various entities. In discovering the ground, hu-

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man beings become able completely to manipulate and transform the various things in nature. In doing so they endow the things with values. Science tells us that only humans, not other entities, have value, and that humans *give* the world its meaning or value as their knowing discloses the world's manipulability. Science is thus humanistic to its core. In its light every particular entity stands neutrally (not as nature) between the necessity of its objective ground (matter in motion, extension, etc.) and the freedom of subjective human art, between fact and value. When understood as the indubitable vantage point for universal, scientific (mathematical) knowledge, subjectivity is the certain and fixed beginning point for discovering the objective ground of manipulable things. But when experienced as the animus of the individual soul, subjectivity is merely arbitrary. Thus, facts are taken to be objective, and values are taken to be merely subjective, thus revealing the essential kinship of subjectivity and manipulable entities, both of which have no fixed character or nature. We cannot hope for salvation from Kant, says Heidegger, because Kant's account of subjectivity—as transcendental unity of apperception *and* as the free legislation of the absolute moral law—itself assumes dogmatically a metaphysical conception of the subject. Thus, free subjectivity and the manipulability of entities turn out to be the same in comparison to the necessity of objective ground. In fact, for modern science there are no essential differences between subjectivity, objectivity, freedom, and manipulability. These aspects of "reality" are actually united in a technological understanding of being: subjectivity is the Archimedean point for uncovering objectivity (Descartes); the doctrine of moral freedom dogmatically presumes that very subjectivity (Kant); and the identification of being with the knowable and changeless entity (objectivity) grew out of the problem of fixing stable grounds for the arbitrary manipulations of human art (Plato and Aristotle). Even for pure natural science "to be" is "to be the ground of the manipulable." Science is humanistic and humanism is technological. (See Heidegger [1927] 1972, 89–101, 202–208, 317–21; 1962, 122–34, 246–52, 364–68; 1982, 112–17; Schürmann 1987, 75; Zimmerman 1990, 157–63, 196, 222–23.)

But our view of the world (including human beings) as manipulable, as the object of control, is not itself within our control. Modern technology is rooted in the metaphysical conception of being that began long ago with Plato. Metaphysics conceived of being in terms of one particular kind, or "domain," of being—objects permanently present before knowing. The task of metaphysics was thus epistemology, which aimed to establish the conditions for certain knowledge of the objects located in the external world. But metaphysics was dogmatic because it merely assumed that we have access to a privileged position outside the presuppositions of a given practical world, because it assumed and thus missed the character of "being-in-the-world," and because it assumed that being is an entity or thing. Metaphysics assumed an impossible independence of theory from practice and confused one domain of being with being itself, forgetting that being neither is an entity or thing nor is identifiable with one or another or even all of its domains, which include objects of knowledge, tools, human beings, the earth, the heavens, and the gods.

Under the sway of metaphysics, the domains of being are so conceived as ultimately to produce the domination of all by one—by objective manipulability. Thus, metaphysics comes to a peak in modern technology, for whose conception of being—which Heidegger calls the *enframing* (*Gestell*)—every entity, whether theoretical object, human being, earth, sky, or god, is taken as the manipulable stuff—the *standing reserve* (*Bestand*)—of an endless industrial business transaction (Heidegger 1977, 17–28).[1] In the full-blown age of technology, the phenomena of art, politics, and the gods are flattened in being understood as the objects of scientific knowledge, in the light of which they become merely useful. In such an age genuine creativity, reverence, loyalty, rootedness, and the full possibilities of astonishment and estrangement are obliterated in a cybernetic swirl that spares nothing, that annihilates everything. The age of technology is the age of the last man, for whom the whole of nature and every human being is the standing reserve of a plethora of industries: publishing, war, travel, entertainment, agriculture, concentration camps, education, and so on. And in this age there are no differences among the great competing social and political systems, all of which are bound together in the embrace of global technology.

Yet despite technology's being the dark night of the world, Heidegger tells us that it is a "danger that saves" (1977, 28–35); for the result of technology is nihilism, the frame of mind that forgets completely the differences among the domains of being and between being and its domains. Nihilism, which in its forgetting says only "It is not" (as in positivism and relativism), actually discloses the negative character of being, the fact that being *is not* an entity or category of entities. It thus discloses that being is the temporal configuration of the separate domains of being—requiring their nearness and farness—and that, as such, being is finite and singular. It discloses that the essence of being is the arbitrary occurrence or happening of one or another utterly unique configuration. Metaphysics, itself an occurrence of being and rooted in everyday experience, hides the essence of being as occurrence. It thus represents the withdrawal of being, which nevertheless always has the power of disclosing itself and thus upsetting our everyday and theoretical understanding of the world and orientations within the world. Technology is the metaphysical appearing of being that ultimately discloses the radical finitude of metaphysics itself.

Technology forces us to see that metaphysics and technology are themselves finite being-dispensations. As such, they are not eternal; but neither are they under our control. Modern technology, the *Gestell*, is but *one* way that being is unconcealed; and

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while it is necessary as a dispensation of being, its necessity is not that of a species' relation to an unchanging genus. This fact lets us account for the uniqueness of modern technology (for the fact that the Baconian–Cartesian project for the mastery of nature is not *essentially* akin to, say, ancient *techne*), despite the rootedness of technology in the metaphysical tradition running all the way back to Plato. The heterogeneous epochs within the metaphysical tradition are each of them new withdrawals of being that undermine the previous metaphysical ontology. But when grasped in the glare of nihilistic technology, the several metaphysical epochs point not to some common ground of human history but to the radical finitude and singularity of every "tradition," including technology and metaphysics. Modern technology is metaphysical and is for that reason a species, or kind, of metaphysics. But the essence of modern technology is to disclose the historical finitude of the very categories of essence, genus, and species (or kind).

When the finitude of being announces itself out of technological nihilism and the exhaustion of metaphysics, we can prepare ourselves for a new relationship to being. This will be one of expecting altogether unpredictable and indeterminate configurations or play of the domains of being. As we are now in the technological release from metaphysics, we can ready ourselves for wholly new occurrences of the human and the divine, the public and the private, the natural and the artificial. These occurrences (the play of being) will be serious, however, because they will always announce the finitude of being and thus shatter an everday way of thinking that for the most part convinces us that being is not finite and that the world is a three-dimensional, scientifically knowable whole that surrounds us. Such readiness for the new is the saving gift of technology over which we have no power. For Heidegger, the challenge posed by technology is not to rediscover our values or invent new ones. Whatever the politics of the future life will be (it will be neither democracy nor communism, which are technocratic), our genuine stance for now is one of expectation, a readiness that holds itself open for new configurations of being whose character we cannot know or force or control.[2]

Now to speak rather generally, Heidegger's account of technology has had influence far beyond those who would identify themselves as Heideggerians. The most powerful recent (and explicitly Heideggerian) version of this argument is expressed in Reiner Schürmann's deconstructive anarchism; but it certainly influenced Arendt's account of modern democracy and liberalism and could be said to have made its mark on the contemporary critics of "foundationalism," for whom getting a bead on politics requires seeing the tradition's search for metaphysical grounds as it has now revealed itself—as the last repository of vain and disfiguring illusion. But setting aside the important question of Heidegger's contemporary legacy, the political implications of his account of technology are clear and familiar enough. They tell us that modern politics, both liberal and socialist, are in the grip of metaphysics. This grip began with Plato and received its decisive technological form in the seventeenth century, which saw the explicit linking of philosophy and politics to the scientific project for the conquest of nature. Modern life, whose roots reach back to classical antiquity, is thus stamped by the technological frame of mind, which so forms modern politics as to distort and truncate the possibilities of human experience. The result is the totalitarian disaster of modernity, in which labor, citizenship, statesmanship, art, and thought—the essential elements of any possible world—are impoverished, if not obliterated, by the very forces we pursue to cure our telltale malaise. Clear thinking shows that we can only hope for the advent of something altogether new and that with the technological self-destruction of the metaphysical tradition we can experience a liberating transformation of political life.[3]

POLITICS AND PHILOSOPHY

What does it mean to say that nihilism announces the end of technology's (and metaphysic's) stamping of an epoch? How do we recognize the pathology of this stamping so that the freedom conferred by its demise can come to light? We can fathom the matter by considering again Heidegger's description of the *Gestell*, a dispensation of being that answers to the productive impetus of human art. The world of this epoch is modeled after the system of (both capitalist and socialist) industrial production, the aim of which is the provision of unlimited goods for the satisfaction of unlimited needs. It is the world formed by the Baconian and Cartesian project for the mastery of nature and the "relief of man's estate." It is a world, in short, where there are no scarce goods, where absolute plenty makes it possible for social and political life to be ordered as a self-regulating, or cybernetic, system.

But why must such a world flatten the human landscape, reduce human beings to mere means, and banish the gods and every mystery? It must do this because to render common life translucent and perfectly orderly it is not enough just to satisfy pressing material needs but it is also necessary to *overcome the need* for goods that are inherently scarce because they cannot be conceived of as falling within the cycle of production, reproduction, and consumption. These are the *noble and beautiful things*, the goods whose rarity and whose opacity to clear definition give rise to perplexity about justice, to serious political competition, and, ultimately, to faith in the gods. Heidegger's accounts of *Gestell* and *Bestand* are illuminating because they suggest that it is openness to the noble and the beautiful things that constitutes, at least in part, the essence of human experience. When we are revolted by the Heideggerian *Bestand*, we supply evidence that such goods exist, that human being is experienced in terms of them, and thus that technology can overcome scarcity only by denaturing

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human life. Moreover, we disclose that the impetus for technological mastery is the desire for justice provoked by the scarcity of such goods, and that the stamp of technology—the domination of utilitarianism, instrumental method, and universal *mathesis*—is the triumph of egalitarian justice: in a world where nothing is scarce because everything can be produced, all can be had and used equally. (It is no accident that Heidegger did not advocate democracy.)

In this light we might question whether the inegalitarian ancients—against their intentions and by way of metaphysics—actually set the stage for such a technological project; for when viewed apart from the Heideggerian interpretation of the tradition, the ancients turn out to have thought more clearly about technology than we are disposed to think and were themselves aware of the link between technology and the tendency of thought to dogmatic metaphysics. To see this, I shall start by turning to Aristotle, who begins the *Politics* with the well-known discussion of the origins of the polis and the distinction between mere productive life and the good life (1252a25–53a1). To appreciate the full meaning of Aristotle's teaching for our problem I shall consider this distinction very carefully, since it is rather less clear than first meets the eye.

At first glance it seems obvious that social life begins with the dividing of labor to meet the simplest and most pressing needs, that is, to provide for mere life. Speaking strictly, however, there is an imaginable alternative to the most rudimentary common life, an alternative considered by Plato (*Republic* 369e–70b), as well as by Aristotle (*Politics* 1253a1–b1; see also Rousseau 1964). This is the life of isolated self-sufficiency. No doubt, for anyone but a god such a life, engaged almost exclusively in attending to the very simplest needs, would be very poor and harsh. But this possibility shows that the really pressing needs are few indeed. And in its light the simplest division of labor comes into being not for the sake of mere life but for the good life. As measured by isolated self-sufficiency, even the very first things produced by society are luxuries that transcend simple need. Thus, even the proximate cause of common life is the desire for goods that lie beyond the most immediate need: human industry is provoked at the very beginning by the desire for the good, as opposed to the merely useful—by the desire for the noble and beautiful things.[4] But while common production is even at its inception drawn forth as production for such goods, it is at that early time close to the all-absorbing leisurelessness of solitary life. The openness to luxury is thus experienced as the press of need. This is, of course, why Aristotle says, with common sense, that political life comes into being first for the sake of mere living rather than for living well.

Yet given the paucity of the very simplest needs, scarcity exists only concurrently with the desire for luxury and the capacity to produce it. Strictly speaking, scarcity could be overcome (and freedom could be possible) not when some level of productive capacity has been reached but only with a limit on the desire for luxurious acquisition. For Aristotle, this limit requires drawing a clear distinction between needlike luxuries and goods that are genuinely noble and beautiful. Therefore, Aristotle follows his account of the origins of the polis with a discussion of the natural limits to acquisition (*Politics* 1256b40–1258a18). For Aristotle, there is such a limit, situated between penury and unlimited acquisitiveness. But this limit is neither easy to see nor realizable by all, because it is in conflict with material production itself.

The reason for this conflict is that if common life comes into being for the sake of the good and not the useful, then the best life would be free not just from the isolate's needs but also from the desire for needlike luxuries that motivates productive life. From the standpoint of such freedom, even the rarest or finest of such luxuries are really not ends (as opposed to useful means), because they are, ultimately, inseparable from the most pressing bodily desires. The best life would not even be the one devoted to the apparently free but really useful activities of politics or moral virtue. Rather, it would be genuinely playful like the life of Socrates, who did not work (though no aristocrat), avoided politics and everything else practical, called the moral virtues vulgar, and eschewed the goods that for almost everyone made life worth living. For Aristotle (*Nicomachean Ethics* 1176a30), as for Plato (*Republic* 500d4–8, 504d3–e3; *Apology* 31a7–33a1), the best life—the one that is an end in itself and useful for nothing—is philosophy, the most uncompromising questioning, wakefulness, and self-knowing. And such a life is unproductive of either useful material things or actions useful for the common good.

By Socrates' own account it was his unstinting questioning of the politicians', the poets', and the manual artisans' claims to practical wisdom (about the noble and good) that got him into trouble (*Apology* 22a–24c). But the leisure of his investigation was at least as controversial as any opinion he might have suggested or criticized. In the final analysis, Socrates' *endless* questioning conflicted with the urgency of all the arts from the productive to the political and poetic. No wonder that he so irritated the practitioners of these arts, to whom he appeared not just unjust and impious but lazy and indifferent to all the goods that for them are the ends of life! It is of utmost importance that in the dialogues we never hear that Socrates, of low birth and poor, performed any productive work.[5] A city of Socrateses is an impossibility; for such leisure as his (one that would attract every moment of waking life) could never be universal, lest production disappear. Consequently, since men live for the good and not the useful and for what is at least possibly within their grasp, most will produce for the sake of needlike luxuries or the performance of useful deeds but will take them as self-sufficient ends in themselves. They will confuse the necessary and sufficient things. And to the extent

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that they live for productive leisure, most will view the truly useless life—the life lived beyond the pursuit of useful luxury or practical virtue and exposing the spurious self-sufficiency of such pursuit—with suspicion if not with anger.[6]

For the very sake of production, human beings must tend to confuse their own useful and productive activities with the noble, beautiful, and good things, whose perfection and self-sufficiency are by nature outside the range of the producible and usable as such. And since these good things are not self-evident, there is a powerful tendency for each art or practical activity to see itself as the self-sufficient, ruling end of the productive order. Even so, no productive art (e.g., medicine, military strategy, moneymaking) can cease to be a useful means to something else, and no virtuous deed can be wholly divorced from service to the common good.[7] No claim to being the end of the productive order is beyond controversy. If production is to occur every one of the productive arts and practical activities (including moral virtue, sacerdotal mediation, and political deliberation) must harbor the potential claim to being an end in itself. In other words, since the goods calling forth the production of artificial means are natural ones, their character of being non-reproducible, useless, and indeterminate results in their being scarce. Consequently, there will always be controversy about their distribution. And since the productive must confuse the good and the useful, all material and artificial means can in practice take on the character of the things that are good by nature. Thus, any order of arts, no matter how productive, will be moved by the attractiveness of goods whose character can rub off on even the most common and reproducible of material means, which can become virtually if not actually scarce. Even in a world where more such means could always be produced, as in a technological utopia, it will be possible (indeed, necessary) for some, convinced of their superior dignity, to claim more of them than others. We cannot assume that in a condition of plenty there will be no serious disputes about the distribution of means. Nor can we distinguish in practice between the goods of the body (means) and the goods of the soul (ends), hoping that when the former are plentiful the latter's scarcity will not matter very much. Rather, disputes about ends will become as contentious as those regarding basic needs in less affluent times, and prosperity will require that we treat plentiful means as if they were natural ends. Such will be the case so long as human society differs from the bees (see Aristotle *Politics* 1235a10; Hobbes 1981, chap. 17).

For Plato, the problem of Socrates disclosed the paradox of human productivity: that the productive must despise the uselessness of the self-sufficient ends for which they work. The ultimate consequence of this paradox is that production transpires within an order that is potentially unstable and given to partisan violence. Aristotle saw things much the same way. He argued that while production requires openness to the noble, beautiful, and good things it also involves the confusion of the necessary and the sufficient. For Aristotle, this confusion leads to the possibility of taking life in an explicitly technological sense, where the good is understood to be the endless, needful acquisition of mere means. Human artfulness thus involves the paradox that in order to get beyond mere life there must be those who take the good to be unlimited acquisition and exchange, that is, mere life writ large. This technological view is inseparable from the counterthrust of the noble and beautiful things, whose scarcity and indeterminateness make political life a serious problem (*Politics* 1256b40).[8] For Aristotle, it is simply not possible for practical life to be completely formed by a particular understanding of the good or to be wholly determined by the technological conception of life that can always accompany such an understanding. To put the matter more generally, for Aristotle, it is impossible for life to be so well determined by knowledge (whether theoretical or practical) that the fragility and contentiousness of politics might be overcome. Both Plato and Aristotle judged the complete stamping of life by technology to be impossible, not because they failed to imagine the project for the conquest of nature[9] but because they thought there were always goods in essentially short supply regardless of our productive capacities.

Now it follows that for the ancients, the fragility, contentiousness, and opacity of political life are not evident to most of those who actually live it. For the most part, certain opinion (not to mention faith) binds political life together from day to day; and citizens live as if that opinion were rooted in a universe that is perfectly clear. But since the truth about the whole is not prior to politics (as if one political opinion could be established on demonstrable grounds), the everyday certainty of political opinion is always haunted by its own finitude and openness and by the unstable partisanship latent in any common good. For the ancients, it is not possible to establish the priority of thought to politics (or vice versa), because political life is at once driven by the need to grasp an elusive whole and (for the sake of material production) intolerant of genuine thought, which is the unstinting activity of doubt and questioning.

Moreover, for Plato there is no human inquiry that does not grow out of the urgency of political life.[10] The truth is an issue only for the being who has language and for whom the truth can matter. And it can matter only because productive life is rent by tensions such as those between rich and poor, noble and base, between public and private life, sacred and profane, war and peace, thought and action, law and morality, and so forth. Where there is no production for luxury (as in the imaginary age of Cronus or the City of Sows), there is no politics and hence no philosophy (*Statesman* 271c–73e; *Republic* 369b–76b).[11] For Plato, politics is the horizon through which the whole occurs. Moreover, this horizon is inseparable from the being of the whole in two respects: (1) the whole *is* as what answers to our need to understand

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the just, the beautiful, and the good; and (2) the whole is intelligible because the problems to which it answers are finite and related to each other as a whole, because in politics there are only so many problems (see *Apology* 22d–e). This is why, although for Plato truth and politics are inseparable, it does not follow that the whole is contingent through and through and thus an artifact that could have an infinite number of alternative forms.

But that the problems of politics are limited does not mean, for Plato, that the whole can be known with sufficient clarity to answer to the needs of practical life (see *Republic* 500d4–8, 504d4–e3, 507a1–5, 509b2–c11; *Protagoras* 329b1–34c6, 349d2–51b2, 360e6–62a4; see also Aristotle *Generation and Corruption* 315a26–16a14). Problems are not answers. Although the whole occurs through the fabric of political opinion that points to it, the whole is beyond the knowledge to which opinion aspires. First of all, while there are only relatively few political problems, the concrete circumstances out of which they arise are never the same. Consequently, if we had a demonstrable grasp of the whole, it would for that very reason be too fixed to give us answers to the specific problems that prompt our attempts to know the whole in the first place.[12] Thus, to know that the noble is superior to the just would not tell us certainly what is in fact noble or just in a given case or whether in the circumstances we should prefer one to the other. Second, for an end genuinely to command or guide our action, that end must be genuinely independent both of our art and our powers of knowing. The existence and natures of such ends cannot be determined by what can be made and understood, lest they be merely ours and created, not "their own." Thus, the whole composed of them does—but yet cannot—occur *as* what answers to our political perplexities. In its appearing it hides itself. And yet we cannot reason from this mysteriousness to the whole's being purely artificial without dogmatically granting to ourselves what cannot be had (i.e., a privileged, epistemological position outside of the presuppositions inherent in the contingent opinions through which the whole appears), that is, without dogmatically assuming about the nature of being that what cannot be known with demonstrable certainty either cannot be or must be artificial. For Plato, the whole occurs only with human experience. But from this fact we can reason no more to constructivist moral relativism than we can to comprehensive teleology. For Plato, the whole occurs—by way of political experience—as a morally inescapable mystery and yet is intelligible as its independent but elusive being shines through the rational capacity to doubt, through our ability to recognize presupposition and dogma.

But again, all this is so only for the most complete wakefulness and questioning, a possibility illustrated (but not "proved") by the fact of Socrates' life. There is a difference between what the whole is—its mysterious presence before genuine questioning—and how it appears to those through whose partisanship such rare questioning occurs. This partisanship is the inevitable result of human productivity. If production is to happen at all, the partisan, unlike Socrates, takes the whole as both knowable and known—so well known in fact that in its light the controversies (and the finitude and fragility) of political life just disappear. Socrates' life is not wholly *strange* to the productive, for they claim to know the good; but it is intolerable to them because it exposes their ignorance of their own ignorance.[13] For Plato, there is a fundamental tension between genuine thought and the inevitable requirement of production—the partisan's need and claim to know. Epistemology is a fact of productive (technological) life, with which Socrates had nothing to do.[14]

For the ancients, that philosophy occurs only with politics does not mean that thought is subordinate to practice, is merely partisan, and is unable to provide guidance to moral life. Independent thought *is* possible, as is shown by the example of Socrates. The life of such thought is the only one wholly free from partisanship; and as such, it is the ultimate model for moderation, without which statesmanship is impossible. Decent politics (not necessarily a genuine common good) is thus dependent on thought that grasps the whole. But this grasp cannot be identified with the cognitive pretenses of epistemology or metaphysics; and its influence can at best be indirect because it can never provide the certainty demanded by productive life. Politics is never as soft and pliable and playful as is genuine thought; to think otherwise would be to imagine life without material production. But at the same time, thought can never do without the spirit of gravity, for two reasons: (1) its own safety will require that playful thought recognize the partisan and deadly moral seriousness from which it arises; and (2) if it is not to succumb to dogmatism, it must be attuned to all of the phenomena of practical life, including those serious things (like the noble, the beautiful, and the just) that can be seen fully for what they are only in their openness to nature, or the *eide*, or the gods. For Plato, thought understood as unadulterated playfulness would be no different from thought understood as unrelieved seriousness, because both stances reflect the dogmatic partisanship essential to production. For Plato, the life of the mind and moderation transpire precariously between two conceits rooted in productive life: the idea that politics *is* ordered after the model of a *knowable* whole and the idea that it *can be* ordered after the model of a *constructible* whole.[15] Both conceits are ultimately technological and dogmatic, since both aspire to find justice in a perfectly adequate supply of the noble and beautiful things, which are in truth inherently scarce and indeterminate goods. For Plato, as for Aristotle, the desire for the technological (and metaphysical) stamp of knowing and making arises out of the very condition (the scarcity of the noble and beautiful things) that renders such a stamp impossible. The ancients did not dogmatically presume productionist metaphysics (Zimmerman 1990, 157–63, 196, 222–23); but, more important, it is possible to see the character

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and limits of technology within their conceptions of being and nature, which do involve the principles of form, ground, and cause.

THE CONQUEST OF NATURE

It may be that classical rationalism does not bear the technological stamp of productionist metaphysics. But such metaphysics could well have been nascent in the ancients, not so much in what they presupposed as in what they did not think sufficiently. It could then be said to have flowered in the modern thinkers, who, while they opposed the ancients in embarking on the technological project for conquering nature, still did so within the categories (form, ground, and cause) of rationalism in general. After all, the crucial modern turn occurred with Bacon and especially (for Heidegger) Descartes, whose project for thought appears in practice in the systems of Hobbes and Locke. In the latter systems we should doubtless see the fundamentals of technological politics: robust utilitarianism, radical individualism, preoccupation with acquisition and production and private property, opposition to social holism and moral virtue, the radical distinction between the state and civil society, aggressive secularism and hostility to the "divine," and the reduction of human personality and community to the typical liberal abstractions. In short, we should see "fallen" everydayness, embedded in spheres of life metaphysically conceived as connected by causal grounds, which could be exploded either by authentic politics or by *Gelassenheit*.[16] Thus, for Hobbes and Locke, commonwealth and citizenship are wholly artificial, intended not to encourage political or moral virtue but rather to reduce the common good to the institutional requirements for the selfish pursuit of long life and demystified, commodious living. These early liberals seem to epitomize the technological hope that politics can be reduced to a machine supporting civil society, that artificial realm where once-conflicting-but-ennobling public ends are wholly privatized and trivialized. Indeed, the character of their politics appears to highlight the technological essence of modern rationalism.

I shall argue that neither Hobbes nor Locke was in the grip of technological thinking, despite appearances to the contrary. Consequently, their politics are not reducible to mechanical technique and in fact require and produce human experience outside the realm of technological, utilitarian calculation. Moreover, they understood the nature and limits of the technology they (especially Locke) did embrace and were aware of the circumstances that led them to their modern avowal of the conquest of nature.

We begin by noting that while Hobbes does contend that politics merely serves civil society, the relation he sees between them is far less technological than first meets the eye. He did not reason from the natural condition (with its free, equal, autonomous individuals moved only by rational self-interest and no partisan or moral conception of the public good) to the spontaneous creation of civil society by all of its members. His political science was directed not to them but, rather, to the few enlightened by the scientific teaching of the state of nature: to him "that is to govern a whole nation" and to the students in the universities, that is, to the clever who would also be the most ambitious (Hobbes 1981, intro., chaps. 21 and 31, review, and concl.; see Tarcov 1984, 42–51). Hobbes knew that at least some individuals in civil society would press controversial political opinions about what have been called the noble and beautiful things. He understood clearly the political importance of opinionated public spiritedness, both as a problem and as an element of a rational political order. Despite Hobbes's clear intention to deflate the character of rank in human life, it is important to recognize just how political, nontechnological, and even noble his understanding of representative government actually is.

For Hobbes, human beings are *not* political animals in the sense that they reach perfection in the activities or virtues of public life. The latter is the clear intention of the doctrine of the state of nature, and any modern liberal must adhere in general to this view. For Hobbes, political life and citizenship are artificial means to the natural common ends of self-preservation and commodious living. In principle, sovereign political power represents individuals not by standing for one or another opinion about the particular ends of life or even by brokering them but, rather, by providing the conditions under which such opinions concern merely private affairs. But even though the sovereign's task is not to determine the citizen's private ends, Hobbes knows perfectly well that any law or act the sovereign judges necessary for the common good of civil peace will tend to favor one particular end rather than another, will answer to some opinion about the goodness or badness of such ends, and will thus benefit some individuals more than others. The task of his political science was to distance political choice from considerations of such ends and benefits, a task accomplished by his distinction between the artificial opinions of the artificial citizen and the natural opinions of the natural individual.

Hobbes argues that if individuals are to escape the prepolitical war of all against all they must submit to absolute sovereign power originating in the consent of each individual. This power is given its particular form (one, few, or many) by the arbitrary decision of the majority of those who consent to leave the natural condition. Each individual, bound to a particular form by the majority, authorizes the sovereign's actions as his own, willed by each as necessary for guaranteeing private contracts and hence for justice and peace. This authorization makes it impossible for the sovereign himself to be accused of an injustice. Therefore, *whatever* the sovereign does or opines in acting to secure peace in a particular instance represents the authorizing individual even if it conflicts with that individual's actual opinion about particular

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ends (Hobbes 1981, chaps. 16–19). In describing the natural condition, Hobbes says that because individuals are equal, each takes his own opinions not just as superior to others' but as applicable to them as well. Thus, in the state of nature the *equality* of individuals inclines each one of them to doubt that any others are equally wise, and the bolder few's love of glory and delight in conquest, as much as general competition and diffidence, are the causes of war (chap. 13). Natural individuals always have opinions about the noble and beautiful things, and these opinions tend to be contentious and political. And since citizenship does not transform human character, individuals continue to harbor such opinions after the establishment of civil society.

Now according to the doctrine of representation, when the ends preferred by a natural person conflict with what the sovereign deems necessary for peace, the sovereign's opinions become the individual's *as citizen* because that individual needs peace more than anything else; otherwise, the individual's opinions remain a merely private matter.[17] The sovereign thus represents natural individuals only by re-presenting some of their opinions, by filtering them through the artificial medium of citizenship. The sovereign's opinions thus do not mirror those of the represented, nor are the sovereign's opinions judged by considering the merits of natural individuals' particular conceptions of the good. Rather, their being accepted as arbitrary (as beyond considerations of justice and any particular good other than peace) is itself an important condition for civil peace. The difference between citizen and sovereign (aside from the sovereign's monopoly of coercive power) is that the sovereign's natural and artificial personalities and opinions are coeval, while the citizen is a natural person whose actions and opinions take on the artificial garb of citizenship only on the occasions when they come under the specific restraints of sovereign will and positive law. In a Hobbesian order, the extent of the positive law will be kept to a relative minimum, for the reasons that Hobbes lays out in *Leviathan*. Once politics is understood as grounded in consent and as instrumental to the natural liberty transpiring in civil society, then sovereigns will be interested in limiting what individuals do as citizens, and individuals will do in civil society what is expected of them when they are not behaving as citizens, namely, mind their own business (which is business).

But this does not happen spontaneously; for the political character of the natural person's opinions tends to cover up the scientifically disclosed teaching of the human heart, namely, the harsh truths that are uncovered by reflecting on the state of nature and that compel us to create the artificial citizen and the private realm of civil society: truths such as the natural right to self-preservation, the war of all against all, and the absence of a summum bonum. Consequently, for Hobbes, the will behind public law (i.e., the sovereign's will) must always be evident and awesome. In order for individuals to let sovereign power represent them, they must always be reminded of the harsh, prepolitical necessity requiring the sovereign's deeds and their confinement to minding their own business. Moreover, the sovereign's lessons must appear through his character as the authoritative measure of "opinions and doctrines," both secular and divine, and of "laws of honor" (Hobbes 1981, chap. 18). The sovereign's will stands in for the science that only a few can know. Thus, the sovereign's will (not his argument or virtue) dampens the political self-assertion to which individuals convinced of their superior wisdom and ends will be tempted. Their occasional experience as artificial citizens must continually remind them to love their natural, spontaneous opinions about the noble and beautiful things less than they fear the artificial, represented counterparts of those opinions.

Yet the fearsome authority of the sovereign is not intended simply to force individuals to mind their own business, against their natural inclinations; for in reminding the individuals of their true self-interest, the sovereign is able to enlist the individual as citizen to attend to the harsh necessities of public life and resolve inevitable political controversies, regardless of the inclinations (one could say "policy inclinations") of the individual's conscience. The energy of representation works both ways for Hobbes, from the natural person to the artificial sovereign and then back from the sovereign to the artificial citizen, who is thus not inert. When natural individuals make a covenant, they authorize the sovereign to "use the strength and means of them all, as he shall think expedient, for their peace and common defense" (Hobbes 1981, chap. 18). It is easy to forget that the doctrine of freedom of conscience—first worked out by Hobbes—was intended not only to prevent the voice of God from meddling in political affairs but also to indemnify the individual before God of any iniquity done as a citizen.[18] The citizen can put his shoulder to the sovereign's wheel, live with the majority's will, or accept a compromise of fundamental moral principle because God judges the conscience of the natural soul, not the actions of the artificial citizen.[19] Thus, even in the case of Hobbes, the representative state is a subtle machine intended not only to manage the partisan tensions of political life but also to establish the conditions for engaging the energies of individuals, as citizens, in this management.[20] This is, of course, a far cry from Heideggerian authenticity. But it is less dependent on the saving power of technology and far more engaging of sacrifice and the political virtues—and thus ultimately of the attractiveness of the noble and beautiful—than is granted by the postmodern account of technology. One might say that it is with Hobbes that we first see described the conditions for political participation—and thus for overcoming the utilitarian–technological calculus of interest—in the modern, Christian age.[21]

This moral depth of liberal doctrine is much clearer in Locke, whose intention in the *Second Treatise of Civil Government* was to present a more politicized version of Hobbes's account of government. For

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Locke, like Hobbes, individuals create political power to escape the disorder caused by their natural equality. Government is established not to secure virtue or glory or some partisan conception of distributive justice but, rather, to guarantee the sphere of private liberty, or civil society. Again, government arises from the consent of each individual, but its form is determined by the will of the majority. But for Locke, unlike Hobbes, government is best able to ensure the broadest scope and tranquillity of civil society if political power is exercised through complicated and restrictive institutional arrangements.[22] As is well known, Locke departs from Hobbes in arguing for the prepolitical, natural status of private property, for the separation of legislative and executive power, for the placing of legislative power in the hands of an occasional assembly, for the subordination of executive to legislative power, and for the majority's freedom for revolution in the face of a legislative or executive threat to private property or civil peace.

However, there is no doubt that for Locke political power is absolute in the Hobbesian sense that there is no law (or power) to which the ultimate sovereign is subordinate. Though it originates in the consent of each individual, the power to make fundamental law resides in the will of the majority—without which "the mighty Leviathan" of commonwealth, to use Locke's Hobbesian words, could not exist for one moment (Locke 1965, pars. 97–99, 132–33). The purpose of this power—the protection of the "lives, liberties and estates" of those who consent to government—is, of course, that power's limiting condition and the limit of those who act as legislators or executives for the majority. But there is no natural juridical principle that specifies the proper form of legislative power, which is determined by what the community (i.e., the absolute majority) "think[s] good" (Locke 1965, par. 132; see also Hobbes 1981, chap. 19). Nor is there any natural or positive juridical principle that specifies when those who represent the majority by making or executing public law do or do not act for the end of political life. There are no such principles that govern the majority's judgment about these questions, aside from the bare fact of its sovereign interest. Hence, there are none that determine the majority's act of revolution against a legislative or executive power seen to have threatened the public good (Locke 1965, pars. 134–35, 142, 168). For Locke, the legitimacy of legislative and executive acts can ultimately be judged only after the fact, by whether they do or do not provoke revolution. Likewise, an act of revolution is judged legitimate only if it occurs, which proves that it is in fact the act of the original majority whose absolute power animates the representative state.[23]

Yet despite this Hobbesianism, Locke distinguished clearly between absolute and arbitrary power. Political power is arbitrary when it is exercised at pleasure and contrary to the common good (1965, par. 137). But the rule of law is not by itself proof against arbitrariness, because (1) law can itself be arbitrary and (2) so much that is essential to preserving the public good, including the very principle of the rule of law itself, is dependent on the exercise of executive power outside the bounds of law. For Locke, rational government depends not simply on the rule of law, the mechanical separation of power, and the majority's freedom for revolution but also on the reasonableness of the few representatives who make law for the majority, the wise exercise of executive prerogative, and the majority's sobriety and moderation. For Locke, private property is the institution best suited to ensure these virtues. It is not the absolute right of individuals to their legitimately held possessions in the realm of civil society (as it is for libertarians). It is, rather, the morally formative institution that both soothes the ambitious few who rule and girds the dignity and political liberty of the majority (1965, pars. 27, 31, 34–41, 123, 131). Locke's Hobbesianism leads him to conclude that rational politics depends as much on the premodern consideration of the character and ends of those who rule as on the modern and technological mechanisms for restraining the abstract and absolute authority of the majority.

It is true that Locke first describes the right to private property as grounded on the natural right to self-preservation and not on any convention or agreement. But it is important to remember that only the most original private property—the simplest appropriation from nature for the sake of nourishment—is determined wholly by individual acquisition and is absolute. Moreover, in the case of the appropriation of land, the natural law prohibition against waste is supplemented by the additional condition that there be enough land left over for others. Although this condition never applies in the first ages of the world (because of the simplicity of needs and means),[24] it establishes the principle that private property in land is not simply absolute and that its legitimacy depends on the welfare of those other than the proprietor. At the very least, in a condition of scarcity the first acquirer would have no proprietary right because all other men would have the right to take what they need by force. Unequal private property, however, is indeed the result of an agreement, namely, the tacit one required for the invention and use of money. The principle on which the agreement is made is the vast increase in general wealth produced by the incentive of unlimited private acquisition. Thus, unequal acquisition by individuals is justified by an appeal to the common good. Moreover, Locke makes it clear that with the advent of civil society, unequal private property in land is settled by positive law (1965, pars. 25–51).

Hence, when Locke says that securing private property is the end of government, he speaks of property broadly as a means for securing the *common* goods of life, liberty, civil peace, and prosperity (see Mansfield 1979). As a means, it is not an absolute end. Rather, it is measured, managed, and limited by those who govern with an eye to the majority's considered judgment about the common good of civil peace. (There is nothing in Locke's understanding of

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property that would prevent a policy of redistributive taxation, so long as it is decided by representation and accepted by the sovereign majority.) For Locke, the governors will be most likely to keep their eyes on the common goods of life, liberty, and estate if they are propertied. And the people (the majority who always have the freedom for revolution) will be most reasonable if it too is "propertied" in a broad sense, that is, if the fundamental cleavage in civil society (between rich and poor) is moderated by the general prosperity resulting from unequal private property. And as regards the individual, private property is an aspect of moral personality that transcends its origin in mere self-preservation. As Locke argues, even though a consenting citizen can be obliged to surrender his life in battle, his inalienable right to self-preservation remains intact in his inviolable property (1965, par. 139). The "personality" appearing in one's property stands above mere life itself and can thus be the medium for overcoming the most immediate self-interest. Far from being the vehicle for licentious selfishness, for Locke, unlimited acquisition in the form of private property actually disciplines the soul.

There is still more to suggest that Locke's politics are far from technological; for he does not think that the liberal order can be constructed mechanically out of separated power, freedom for revolution, and the spontaneous moral and political efficacy of private property and unlimited acquisition. As I have argued elsewhere, Locke's distinction between the rational and industrious, on the one hand, and the quarrelsome and contentious, on the other, cannot be identified simply with the difference between the propertied rich and the propertyless poor (Weinberger 1988a, 1988b). Rather, Locke's account of human production makes it clear that the politically contentious passions and opinions (those having to do with power, honor, and vain ambition, all engendered by the call of the noble and beautiful) spring from the very desires and capacities of mind that give rise to the division of labor and expansive material production. The invention of money depends on the human capacity to confer value on useless or "fantastical and imaginary" things, as does the desire to produce beyond the simplest necessity; and both requirements of production lead to what Locke calls "evil concupiscence" and "vain ambition" (1965, pars. 46, 111, 184). In fact, the very artfulness of the "industrious and rational" is for Locke the ultimate source of political self-assertion. For Locke, the important political distinction is not just between rich and poor but also the Machiavellian one between peoples and princes. This is the distinction between those who humbly mind their own business and those who—moved by the attraction of the noble and the beautiful to contentious opinion, ambition, and the love of glory and honor—do not. The princely types may well stir up the poor, but they are themselves unlikely to be poor or to focus their ambitions simply on the claims or discontents of the poor. These princely types are the *busy heads* that Locke says can be found in any political order devoted to private property, material acquisition, and the freedoms of civil society (1965, par. 230).

They are not, however, an unmitigated evil according to Locke; for they provide the solution to the real problem of limiting government, which is how to engage the people's freedom for revolution. At first glance, the problem seems to be that revolution is so very dangerous. It is thus reasonable to be silent about the *right to revolution* (Locke never uses the term) and perhaps even to deny it altogether. But such caution is not necessary, says Locke, because the people, not being princely, are very slow to be moved. The real problem is not the people's volatility but their inertia, which inclines them to act only when their liberties have already been seized and it is too late to do anything about it. Something must goad the slow people to revolutionary vigilance. The only candidate for this role is the ambition of the well-heeled, busy heads who do not happen to rule (1965, pars. 220, 223–25, 230). But since there is no natural or positive law prescribing its precise conditions, revolution can never be a legal act—in which case the service rendered to the common good by the ambitious few is always in the form of conspiracy against legal—and, prior to the revolution, legitimate—authority. Most such conspiracies (which are always around in civil society) will fail, with the conspirators answering both to "God and Man" (1965, pars. 204–209, 240–42). But if one should succeed, it is only because the conditions were such as to incline the people to be moved on time. These are the conditions that (always after the fact) prove the legitimacy of any revolution. For Locke, the essence of the liberal state is the separation of politics and civil society. But this separation is effected only by engaging and managing the political forces arising from civil society and ultimately from the essence of human production. The secret of modern government is the subtle mixture of separated power, the moral and political institution of private property, and the revolutionary exploitation of conspiratorial ambition: the wise use and abuse of the princely on behalf of the political assertiveness and virtue of the people. Again, we could say that with Locke's account of property and revolution we see a fuller account of the conditions for political virtue and self-transcendence in the modern age.

POLITICS AND RELIGION

From Hobbes and Locke we learn that the modern political order is technological in the sense that it sees political practice as but a means to some transpolitical (hence ultimately private) end. Moreover, we learn from Locke that the liberal order is technological in the further sense that it manipulates those who do not fully know themselves, namely, the ambitious, who act for ends, ultimately private, that they do not intend. Yet despite this technological character of Hobbes and Locke's politics and their explicit opposition to classical political and moral doctrine, in both

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can be seen a lesson hearkening back to Platonic political philosophy, namely, that politics is rooted in the inevitable existence of goods—the noble and beautiful things—whose scarcity and indeterminateness are coeval with human artfulness no matter how efficient or productive it might be. The technological impetus is the very source of the tensions of political life; thus, it is never possible for technology by itself, apart from some kind of moral and political virtue, to resolve them.[25]

If we find no complete technological stamp on their politics, we should at least be wary of concluding that these modern founders, who attacked the universals of ancient and scholastic metaphysics, dogmatically assumed a *technological* metaphysics of their own.[26] We might then consider that the contradictions often noted between their politics and their skeptical metaphysics were not unconscious and that their foundationalism (as expressed in the state of nature) did not reflect a dogmatic understanding of being. Indeed, it rather seems possible that like Plato and Aristotle, they understood how human thought transpires within the urgent political problems generated by common production. They knew, likewise, that thought is always tempted to the service of dogmatic partisanship and faith and that the activity of genuine questioning and doubt is fragile and rare. At least in part, it was this knowledge that led them to attack the dogmatic metaphysics and enthusiasms of the schools. It would be reasonable, I think, to read Hobbes and Locke's theoretical works in the light of an astounding and almost-always-ignored remark made by Hobbes: in the *Leviathan*, while summing up his argument against the universals of Aristotelian metaphysics, Hobbes says that Aristotle may have known his "entities and essences" to be "false philosophy" but "writ it as a thing consonant to, and corroborative of [the Greeks'] religion; and fearing the fate of Socrates" (Hobbes 1981, chap. 46). Here Hobbes tells us that Aristotle's "first philosophy" might have been ironic, moved by a self-conscious understanding of the relation between thought and politics—akin to Plato's noble lies and myths. If so, then Hobbes suggests that his own "first philosophy" was directed not at Aristotle's errors and toward an alternative, demonstrable first philosophy but, rather, at an earlier philosophic rhetoric occasioned by the clash between the gods (the guarantors of the just distribution of the noble and beautiful things) and the activity of thought. Given this remark and the subtlety of his and Locke's politics, we have at least to consider that far from having been in the grip of metaphysics and technology, the first liberals turned to the scientific conquest of nature with a clear understanding of that project's rootedness in the problematic character of productive life, with a clear understanding of its theoretical limits and practical dangers and with an explicit concern for what they took to be the peculiarly modern problem of politics: Christianity.

For the ancients, faith is inevitable so long as justice is the overarching political virtue, for only the gods can assure that injustice does not pay. And justice *is* the overarching political virtue because production generates partisan claims about a supposedly common good. But this meant that thought (in its purest form unproductive) is an activity that cannot be taken as just (or good) by any practical standard. Thus, if the life of philosophy is the ultimate measure of good politics (of a virtual, if not a genuine, common good), it could be so only as the distant model of a practical virtue that is itself not simply identical with justice.[27] For the ancients, only such practical virtue could restrain the dangers posed to common life by partisanship and its attendant faith in the gods. Given the fragility, instability, and dubiousness of such virtue, the ancients were pessimistic about the likelihood of decent politics. The founders of the modern liberal tradition sensed a new situation posed by Christianity: because of its universalism, Christian faith was even more dangerous to thought and common life than paganism; and because of its (egalitarian) doctrines of sin and conscience it was resistant to an Aristotelian political science based on the pagan moral virtues. Fully aware of the problem of Socrates and anticipating the extraordinary political power of modern science, they thought it best to replace Socratic irony and Aristotelian politics with dogmatic, scientific rationalism as the nexus between the life of thought and politics.[28] In this crucial respect, at its peaks the modern liberal tradition is conspiratorial rather than epistemological, dogmatic, and technological. The early moderns thought they could do much better than the ancients in managing partisanship and belief. But like the ancients they displaced, the liberal founders did not think it possible or desirable that technology wholly stamp practical life. Of course, they counted on technological progress and the acquisitiveness of civil society. But for this very reason they also counted on *liberal* technological politics, which required its own version of moral virtue and made considerable room for thought within the rigid certainties of the new technological age.

NATURE AND THE LIMITS OF TECHNOLOGY

I do not claim that modern rationalism and politics are simply free from metaphysical and technological dogmatism. Nor do I deny that the turn from ancient to modern thought was of enormous moral and political significance. I do mean to suggest that some architects of modern politics (especially the early liberals who thought most deeply about the Baconian project for mastering nature) understood that neither reason nor any system of technical means can ever *completely* stamp an age or political arrangement. And I do mean to suggest that they shared this grasp of reason and technology with their ancient rivals. The best of the early moderns knew that political life requires some form of practical virtue and that such

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virtue is always in tension with the strictest demands of the life of reason. They knew that the dogmatism required for practical life conflicts with the fullest capacity for doubt and questioning and that this conflict would abide in and mark the technological age which political circumstances required them to promote. Thus, I mean to suggest that from these modern architects and their rivals one can distill an alternative to Heidegger's understanding of technology and then find an alternative version of Heidegger's view of how the ancients and the moderns are at once the same (as metaphysicians) and radically different (as regards the modern age of *das Gestell*).

Human experience is characterized by artfulness that is moved by attraction to goods (the noble and beautiful things) that are by nature scarce. Why are they by nature scarce? It is because as the independent and authoritative ends of art—as the objects of human longing—they are ultimately beyond our productive grasp. Moreover, the attempt to identify concrete things, here and now, as unambiguously beautiful and noble—as determinately good—never succeeds altogether. It is always possible to see a problem in every such attempt. Therefore, each such noble and beautiful thing points to what is good in itself and thus beyond any world here and now. And what these things are in themselves and beyond any world cannot be determined wholly by our knowing. But at the same time, the elusiveness of such ends and the urgency of material production combine to make human life impossible if art and convention do not claim to know and possess these ends. Their possession is impossible apart from production for luxury, which itself happens in part through the efforts of those for whom the noble and beautiful things consist in the activity of unlimited acquisition. And as a consequence of the essential scarcity of the natural goods, human life is always riven by the myriad tensions among the many and the few—between democratic and nondemocratic conceptions of the noble and beautiful.

These elements of experience produce (in addition to religious faith) two dogmatic conceptions of the natural whole, which, in each case, is taken to be so clear as to provide guidance about the just distribution of the noble and beautiful things. The first lets us know precisely what the rare noble and beautiful things are and which few should have them; the second lets us know that since the whole is a figment of human convention, there are no noble and beautiful things apart from those we freely create. In both cases, the noble and beautiful things are thought to be in perfectly adequate supply—either because there are just enough to fill a limited need or always enough to fill an unlimited demand. In the first case is the argument from strict teleology for the aristocrat; in the second case is the argument from constructivist relativism for the democratic–egalitarian proponent of the life of unlimited acquisition and exchange.[29] In mistaking the problematic character of the noble and beautiful things, both conceptions reflect the partisan's desire for justice and for overcoming the essential tensions of political life. Both are unphilosophic. But since philosophy is in tension with productive (i.e., technological) life, it is not possible for the two conceptions to become philosophic—to come to see their own limitations and problems. Consequently, an essential task of politics is the management of these two inevitable, irascible, and dogmatic tendencies of human reason. One or the other will always predominate in practical life. In the Christian age (we learn from the first moderns), it will have to be the second. And of course (as in every age), contingent circumstances require that thought practice the rhetoric most suitable to the times.

From this point of view I conclude, against Heidegger, that the problem of technology can be understood best in essentialistic terms—in terms of cause (or ground), genus, and species. Considered generically, the problem of technology identifies the tension between production and the self-sufficient ends that set production in motion and thus the political character of any order of human art; and the problem of technology emerges as the problem of nature, that is, how we are to live with the inevitable political tensions (finite in number and coeval with human life) caused by our technical openness to authoritative goods beyond our artful and cognitive activities (i.e., beyond any given conventional world). Considered specifically, the contemporary problem of technology springs from the decision to use the democratic–egalitarian claims of unlimited acquisition and theoretical constructivism to manage the natural tensions of common life. In this narrower respect, our contemporary problem of technology comes to light as the *exaggerated* conceit that common life can be stamped wholly by reason and by a system of productive means (a conceit used and abetted but never naively believed by the early liberals). The modern problem of technology is the inevitably partisan dream—now heightened by the project of scientific conquest—that the natural fissures of political life can be eliminated by an achievement of human thought and art.

At this point, the obvious objection is that to follow this line of thinking we would have to take the phenomenon of nature seriously; that is, we would have to take seriously the existence of a finite number of essential human problems and the existence of worldly and transworldly objects that call these problems into being. We would have to make a case for things like human nature, a normative highest good, and the Platonic *eide* and Aristotelian causes (perplexing as they are), lest this account of technology be nothing but a story that is at once dogmatic in ignoring Heidegger's powerful account of the worldhood of the world and dogmatically moralistic in simply assuming that there is some natural standard (the life of questioning) by which we can gauge our management of political life. Of course, I cannot refute this objection here. Suffice it to say that Heidegger's powerful attack on the epistemologically privileged position sought by some in the metaphys-

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ical tradition (a position outside the assumptions of some contingent, lived world) does not by itself uproot the question of the being of nature. Rather, I would suggest that it liberates this question from the dogmatism and moralism of Enlightenment rationalism, which still lives secretly even in much post-Heideggerian thought.[30] Heidegger helps us reconsider the relations among thinking, knowing, being, art, nature, and politics, without dogmatically reducing them one to the other or presuming the character of being-in-the-world. In this respect, he helps us look for the grounds that are common to the ancients and the moderns, so that we may better understand how they differ. But if we can recognize a categorical and causal account of being that is not dogmatic from the beginning, how are we to choose between it and the Heideggerian alternative? Perhaps we may do so only by asking which one permits the fullest awareness of the character of practical life in all its comedy and gravity. To choose for our position, we would have to show two things: (1) that such awareness *requires* our granting the perplexed and dangerous openness of political life to governing entities whose existence lies beyond any worldly power to know or make—that without such a causal yet skeptical understanding of being, we cannot be fully open to the political phenomena that provoke our questioning wonder; (2) that such phenomena cannot be outstripped (always running ahead of every project to overcome them), so that we cannot escape the need to see them for what they are.

In other words, we would have to show that Heideggerian being, which grants a causeless and "factical" play of its domains, cannot account for the genuine gravity of political life—for how the elements of experience contend against each other, as we see in the challenge of thought to faith, the tension between private and public life, the conflicts between morality and politics, the difference between the good and the just, and so on. We would have to show that such contention is possible only insofar as their elements are related causally and hierarchically, so that each by its very nature claims an authority, beyond the contingencies of any given world, to order the others. And we would have to see that however much the fact of such contention calls forth our efforts to overcome it by way of making and knowing, both making and knowing are even at their best the very source of this contention. Nature, as I propose to think about it, is beyond any project for conquest. Technology could, of course, simply destroy the natural soul by making it either subhuman or godlike; but it could never wholly stamp the *human* species because it cannot supply all of the needs that the soul has spontaneously (or by nature), such as the desire for noble preeminence.[31] Consequently, the harder technology presses, the more intensely we sense a "problem" with it. I am suggesting that the problem of technology is most fully understood when we approach it through the old-fashioned question of natural justice that transcends any given political conventions. I am thus suggesting that no era's thinking and practice is so finite and self-contained that it can be wholly stamped by technology and that we do not have to recur to Heideggerian being to see the limits of the stamp. But I am also suggesting that such direction as nature gives to our groping for justice will never satisfy the demands of everyday politics and morality; for that direction consists in the limited extent to which the widest opening of our eyes can cure the blindnesses of political life.

All this is to say that we can choose against Heidegger only by showing that our view inclines us best to see unblinkingly the fissures within political life and to resist the dogmatic partisanship that is inseparable from *techne* and *poiesis*. Is it possible that for all its extraordinary power, the Heideggerian attempt to grasp the character of thought and art results in an equally extraordinary obtuseness toward the nature of political life? I would argue that both anticipatory resoluteness and *Gelassenheit* do just this: the former tempts us to a spurious unity of such things as work, thought, and war; and the latter tempts us to the spurious disconnectedness of these same phenomena.[32] Is it thus possible that Heideggerian being is itself a danger of technology, which always (like morality) tempts us vainly to try to jump out of our political skins? My argument is that if we own up to what it means to be human, the answer is a provisional *yes*. But if we need further proof that the homely problems of political life just will not go away, that there really are not that many of them, and that we deny this at our peril, perhaps we need only relearn how to look right under our noses.

There is nothing more technological than the idea that a metaphysical tradition—some rational system—could wholly stamp an age and its practical life. This is, however, just what Heidegger's account of technology would have us believe. And yet Heidegger's accounts of *Gestell* and *Bestand* do help us to think about what is so disturbing about modern technology—its tendency to deny the objects of the soul's desire, the noble and beautiful things that are in essence both rare and elusive. In thinking about technology, we discover that not the stamp of metaphysics but the political problems within which thought arises out of production is turned toward a mysterious but necessary whole and is always precarious. Technology is indeed a danger that saves: it compels us to think anew about the meaning of nature. But if, in grasping technology, we owe anything to Heidegger (and we certainly do) it is that Heidegger helps us to see how technology turns us away from Heidegger.[33] I suspect that such a turn will require us to admit that we cannot escape the question of the highest good and that while neither politics nor justice is that highest good, we cannot think it apart from the moral demands for justice that frame political life and any questioning about being and the soul.

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Notes

This essay grew out of work begun some years ago when I held a Fellowship for Independent Study and Research from the National Endowment for the Humanities. I thank my colleagues in Michigan State's Symposium on Science, Reason, and Modern Democracy, Arthur Melzer and M. Richard Zinman, for their help. Mark Blitz, Werner Dannhauser, and Harvey Mansfield all read versions of the essay and gave much useful advice.

1. The *Gestell* is not a "concept" but, rather, the particular configuration of the domains of being that frames our age; but I risk distortion here for the sake of brevity. For the argument so far and for what follows, see also Heidegger's essays *Das Ding* ("The Thing") (1971, 165–86; 1967, vol. 2, 37–55), *Wissenschaft und Besinnung* ("Science and Reflection") (1977, 155–82; 1967, vol. 1, 37–62), and *Die Zeit des Weltbildes* ("The Age of the World Picture") (1977, 115–54; 1950, 69–104).

2. For an excellent summary of Heidegger's account of technology and nihilism, see Gillespie 1984, 124–64; see also, among many others, Kolb 1986, 144–200.

3. See Schürmann 1987, 17, where he explains the concept of stamps (*Prägungen*), and Arendt 1958 and 1963. For instance, without being a Heideggerian, Richard Rorty (1989) accepts completely Heidegger's account of the tradition. It would take a long time to account for the Heideggerian influences on the various contemporary schools of thought (both liberal and democratic) that see technology or metaphysics or both as a fundamental problem for modern politics and to relate these influences to the critical movement (i.e., Kant) within the tradition itself.

4. I do not here deny the distinction between political and prepolitical social life (i.e., between village and city). On the contrary, I mean to suggest that by contrast to isolated self-sufficiency all prepolitical social forms are defective as judged by their own principles. In other words, as regards political life Aristotle's teleology can be stated more even forcefully than Aristotle states it. Perhaps he demurred because he knew that what applies to the village can apply to the city, as well.

5. His political service came only when required; and we hear his courage described only in retreat (Plato, *Apology* 23b–c, 30c–33c, 36b–38c; *Symposium* 220d–21c). And, of course, his leisure made him dependent if not on those who work, then on the rich nobles with whom he associated.

6. But at the same time, such a life will never be altogether alien to them.

7. On the debatable status of moral virtue, see *Nicomachean Ethics* 1103a14–b25, 1105a17–b18, 1179c34–81d24; *Politics* 1281a11–84b34, 1323b21-24a35.

8. It is not true that technological conceptions of life and nature are altogether new. On the contrary, Aristotle teaches us that they are coeval with human life.

9. Plato's account of Atlantis in the *Timaeus* and *Critias* and Aristotle's discussion of the possibility of unlimited acquisition and Hippodamus should put this view to rest. Aristotle says that if tools were to move by themselves (on command or by anticipation), then master craftsmen would have no need for subordinates and masters would not need slaves (*Politics* 1253b34–54a1). Aristotle's meaning here is not what Marx took it to be (that the machine would minimize work; Marx 1977, vol. 1, pp. 531–32) because Aristotle speaks not just of machines but of miraculous, "thinking" machines that could be moved by speech or intention alone. But even granted such machines (which now seem possible), they would either still require much human work for their production, maintenance, and improvement or else (were they entirely self-moving) their capacities would raise serious questions about the justice of *their* enslavement. Moreover—and this is the important point—their productivity would not overcome the scarcity of the noble and beautiful things, the desire for which moves production in the first place.

10. This is why he wrote dialogues and why there is no Platonic doctrine presented apart from some consideration of useful myth or political necessity. Nowhere, in fact, does Plato present a demonstrable proof of the existence of the *eide* or of the existence of some transcendent reality that grounds the contingent phenomena of life and language. Plato's dialogues are first and foremost hermeneutical, not epistemological; and they always end in doubt, not wisdom.

11. The City of Sows' predecessor—the City of Utmost Necessity, where labor is first divided—is as imaginary as the age of Cronus, because that city's bare necessity is, in fact, luxurious by comparison to the alternative, rejected by Adeimantus, of isolated self-sufficiency. Thus, the City of Sows is, in fact, the adolescent form of its luxurious (and political) successor, a fact hinted at by the existence, even in the City of Utmost Necessity, of animal flesh (the by-product of the shoemaker's art), apparently introduced only after Glaucon's demand for luxury.

12. Recall Aristotle's perplexing claim that while there is such a thing as the just by nature, it is always changeable (*Nicomachean Ethics* 1134b18–35a15).

13. It is especially intolerable, since such ignorance regards the nature of the gods, who guarantee the justice of living for the productive common good, by whichever partisan conception it is defined (Plato *Apology* 23c2–30c1).

14. This contradicts, I should say, Richard Rorty's claim that Socrates was a metaphysician in the sense of having *assumed* that a "term in his final vocabulary" (i.e., in his account of ends) refers to something that has a real essence (1989, 74). See n. 3.

15. The former is the view of "traditional morality," as exemplified by Socrates' tormentor Anytus; the latter is the view of the sophists and rhetoricians, to whom the powerful of Athens sent their sons for instruction (see Plato's dialogues *Meno* 70a1–71e1, 80d5–81a21, 89d–93a4, 95a2–96e5; *Apology* 18a7–21b, 23c2–24c9; *Sophist* 234a7–36d3; 240d1–41c6, 260d5–61b4; *Republic* 492a1–d1, 595a1–99a4; *Gorgias* 465b6–c7, 482c3–86d1, 494a6–97d8; cf. Aristotle *On Sophistical Refutation*, 183a36–84b9; and *Rhetoric* 1355a3–b34).

16. See, e.g., Heidegger [1927] 1972, 126–30, 175–80; 1962, 163–68, 219–24; 1933; 1976. Falling, however, is not to be understood as an ontic moral condition "caused" by the likes of Hobbes. As Heidegger insists, it is a "definite existential characteristic of Dasein itself" (1962, 220).

17. The liberty of the natural condition is the same in character as the liberty of the subject; it is just that the subject's liberty is narrowed by civil law for the sake of maximizing the scope of private (natural) liberty (see Hobbes 1981, chap. 21).

18. See Hobbes 1981, chaps. 21, 42–43; the sovereign can commit an iniquity—but not an injustice—against a citizen; and "profession of the tongue is but an external thing" (see Orwin 1975).

19. It is only the sovereign for whom natural and artificial personality are inseparable and coeval. Thus, if there is a God, the sovereign takes the biggest risk that his conscience might be wrong. Indeed, he takes the sins of the citizen upon himself!

20. It especially engages the energies of the "enlightened." Hobbes clearly presumed the political ambition of the enlightened few as essential to a rational political order. Given the teaching of the state of nature as he presents it (showing political opinions about the noble and good to be in every case spurious), one suspects that Hobbes thought the enlightened to exhibit their own special lack of self-knowledge. Like Socrates, Hobbes never ran for office or aspired to rule.

21. This is not to say that the likes of Hobbes and Locke agree with the ancients about the character and status of the moral virtues and the noble and beautiful things. They do not; and to think otherwise would lead us astray. But I do mean to suggest that the ancients' "belief" in the existence and intelligibility of such phenomena and the moderns "rejection" of them are much more complicated, subtle, and qualified than is assumed by the critics of technology.

22. Hobbes, of course, preferred absolute monarchy. However, it is often forgotten that for Hobbes all forms of

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government (one, few, or many) are equally representative and equally absolute (1981, chap. 19).

23. Like Hobbes, then, Locke's understanding of representative government leaves no room for individual or collective appeals to conscience—or to higher law and natural distributive justice—in evaluating the form or particular policies of an established political authority. Representative political power is limited (kept to its task of preserving the sphere of civil society) by the freedom for revolution, rooted ultimately in the absolute will of the majority. The majority's will determines the individual wills of those who originally authorize it, and that same majority's absolute judgment confronts and limits the particular wills of legislators and executives. But again, there are no juridical principles that govern the exercise of this judgment.

24. I doubt, however, that Locke really thinks it possible that there ever was such a first age.

25. As Hobbes says (in subtle agreement and disagreement with Aristotle), men differ from bees and ants in competing for honor and dignity; in relishing "nothing but what is eminent," which generates the tension between public and private good; in thinking themselves wiser in public business than others; in misrepresenting good and evil; and in creating government (1981, chap. 17).

26. This is the clear implication of Heidegger's account of Hobbes's nominalism (Heidegger 1982, 183–92, 200–208).

27. According to Aristotle, genuine courage requires that the courageous man *not* believe in life (and thus punishment or reward) after death, piety is not one of the virtues, and justice competes with magnanimity for the status of highest virtue. There is, then, a tension between virtue, on the one hand, and faith and justice, on the other, as well as between faith and thought. This is not to say, however, that the virtuous are easy friends of thought or philosophy; for virtue is based not on thought but on habit, and neither Aristotle nor Plato thought the good aristocrats to be any less dogmatic than the faithful and just many (see *Nicomachean Ethics* 1100a10–1101b10, 1103a15–26, 1115a25–33).

28. A further such nexus between thought and politics was the typically modern doctrine that justice is the highest virtue if not, indeed, the only principle of morality.

29. This explains how the aristocratic but constructivist Nietzsche could be assimilated so easily to contemporary egalitarian politics; that is, constructivism is always secretly democratic and egalitarian, so that it is no surprise that Nietzsche finally described himself as a saintly aristocrat, that is, one who cares about, and bears responsibility for, the whole of humanity.

30. Imagine the likes of Socrates or Hobbes or Locke confronting Heidegger or a contemporary advocate of "postmodernism." Would not the former immediately wonder of the latter what kind of partisan, moralistic ax was being ground in the claim that political life would be transformed when "metaphysics" or "foundationalism" were left behind? Would they not wonder whether the real issue at stake was not a naive hopefulness (often hidden beneath realistic claims about the character of power) that political life would give itself over to unity once we gave up the hopeless quest for a demonstrable, unifying ground? And would they not say that this hopefulness would founder on, if it did not exacerbate, the timeless conflicts between the many and the few, the rich and the poor, the noble and the useful, the good and the just?

31. Of course this desire can take technological form (e.g., producing with the least effort the very best robot), but no technology can make preeminence available to all.

32. For uncovering the questions in this and the previous paragraph, I am indebted to Mark Blitz's original and excellent discussions (1981, 169–80, 210–22).

33. Of course, that Heidegger might be wrong does not mean that the tradition as I have interpreted it here must be right. Most contemporary theorists of democracy and liberalism are not Heideggerian, nor are they inclined to think that they depend on taking seriously the questions of nature, the whole, and the highest good. To make my suggestions stick would require showing that we need them to make sense out of contemporary liberal and democratic politics—especially given the recent spate of "antifoundationalist" and (nonmetaphysical) neo-Hegelian defenses of liberalism (e.g., Pinkard 1987, 1988). I think we do need to follow up my suggestions in order both to defend and to see the now-often-denied problems of liberal politics. But these are themselves enormous questions.

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THE PRICE ELASTICITY OF MASS PREFERENCES

DONALD PHILIP GREEN *Yale University*

I compare the price elasticity of economic and political preferences. My central hypothesis is that willingness to pay, whether expressed verbally or through cash transactions, is more price-elastic for economic consumption goods than for public goods. I find that increases in price greatly diminish the proportion of people willing to pay for consumer goods, such as housing or hardback books; whereas the proportion willing to pay more in taxes to support a public good, such as environmental protection or shelter for the homeless, is much less responsive to changes in price. I conclude by discussing the theoretical and political implications of willingness to pay for public and private goods.

> Voters and customers are essentially the same people. Mr. Smith buys and votes; he is the same man in the supermarket and in the voting booth.
>
> —Tullock, *The Vote Motive*

One of the most provocative findings in public opinion research is that tangible personal interests are seldom correlated with policy opinions. The opinions of the employed and unemployed differ only slightly on issues such as whether government should guarantee employment or expand public works programs (Sears et al. 1980; Schlozman and Verba 1979). Much the same may be said of the differences between parents and nonparents on the issue of busing (McConahay 1982; Sears, Hensler, and Speer 1979), men and women on women's rights or the Equal Rights Amendment (Mansbridge 1985; Shapiro and Mahajan 1986), and the medically indigent and those with health coverage on the question of national health insurance (Green 1988; Sears et al. 1980). True, homeowners are at times significantly more receptive to property tax reductions than renters (Sears and Citrin 1985), and smokers more hostile to cigarette taxes and smoking restrictions than nonsmokers (Green and Gerken 1989). But such discrepant cases are overwhelmed by evidence showing the limited influence of narrow self-interest on opinions toward policies such as bilingual education (Sears and Huddy 1987), affirmative action (Kinder and Sanders 1990), energy conservation (Sears et al. 1978), escalation of the Vietnam War (Lau, Brown, and Sears 1978), income redistribution (Kluegel and Smith 1982; Verba and Schlozman 1977), and social security (Beck and Dye 1982; Lau and Sears 1981). In sum, although policies are invariably more costly to some individuals than to others, personal costs and benefits are poor predictors of how people wish government to act.

Such findings run counter to the widely held assumption that political preferences are rooted in selfish and materialistic motives, an assumption that turns up in the writings of Aristotle, Locke, Madison, Marx, Schumpeter, and others (see Green 1988; Mansbridge 1990). But what makes these results especially intriguing is that self-interest seems to fare quite well in other disciplines, notably economics. The empirical literature on the behavior of consumers and firms is replete with illustrations of how ordinary people respond to prices in an effort to buy cheap and sell dear (Bolton 1989; Monroe 1979; Tellis 1988). Even scholars who emphasize the moral or affective dimensions of economic behavior (Collard 1978; Etzioni 1986, 1988) acknowledge that as a rule, a rise in price reduces quantity demanded.

However plausible it may be to suppose that direct personal costs have more force in economics than politics, one must be cautious about comparing the empirical work in the two disciplines. There are important methodological differences between economists' and political scientists' ways of assessing the influence of tangible personal costs. Economists tend to examine the extent to which price changes affect aggregate sales, whereas political scientists tend to study how individuals react to changing incentives. Moreover, economists typically study situations in which prices are stated explicitly to consumers, whereas in public opinion research voters are left to figure out for themselves what a policy will mean for their pocketbooks. Thus, in the absence of a direct empirical test, one cannot rule out the possibility that the apparent contrast between economic and political decisionmaking is to some degree artifactual.

I shall attempt a systematic comparison of the price elasticities of economic and political preferences. I begin with a discussion of the definition and measurement of the key terms in this study: *willingness to pay, asking price,* and *price elasticity*. Using a combination of survey data collected between 1940 and 1990, experimental evidence about consumer behavior, and records of employee contributions to the United Way, I assess the hypothesis that willingness to pay (whether expressed verbally or through cash transactions) is more price-elastic for economic consumption goods than for public goods. Small increases in price are found to reduce sharply the proportion of people willing to pay for consumer goods. By comparison, the proportion willing to pay more in taxes to shelter the homeless or protect the environment is less responsive to price changes. I consider a number of potential explanations for the contrast between public

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and private goods, then conclude by discussing some of the political implications of the electorate's inelastic demand for public goods.

CONCEPTUALIZATION AND MEASUREMENT

Willingness to Pay

Research on the public's willingness to pay for various goods and services spans several disciplines. Market researchers ask people how much they would be willing to pay for new products. Pollsters interested in the popularity of certain policy initiatives test the depth of public support by asking citizens whether they would be willing to pay more in taxes in return for certain government services. Students of consumer behavior measure the price ranges that shoppers find acceptable for various products. Economists, too, have employed survey research methods in order to assess the public's contingent valuation of various public, or nonmarket, goods.

Notwithstanding the frequency with which it is used, *willingness to pay* remains a somewhat ambiguous construct.[1] The fact that someone verbally expresses a willingness to pay a given price for a certain good implies neither that a purchase at that price is imminent nor that the individual intends to make the purchase in the future. In this sense, verbal expressions of willingness to pay are distinct from behavior, as well as from behavioral intention. Nevertheless, willingness to pay is thought to have behavioral implications. In the economic realm, one would expect those who say they would pay $50 for a hand calculator to be more likely to make such a purchase than those who express unwillingness; in the political realm, one would expect those expressedly willing to pay $50 more in taxes for local schools to be more likely to vote in favor of such a ballot measure than the expressedly unwilling.

Just how well behavior may be predicted by willingness to pay is a matter of considerable disagreement. Some scholars regard utterances of this sort with suspicion, arguing that people have strategic incentives for concealing or misrepresenting their true behavioral predispositions (Samuelson 1954). Economists, in particular, opine that in the absence of real budget constraints, talk is cheap, and purported willingness invariably outstrips effective demand. Another common criticism is that people have strategic incentives to understate their willingness to pay "if they believe they will actually have to pay the amount they reveal, and believe also that there is a good chance the good will be provided even if they understate their true [willingness-to-pay] amount" (Mitchell and Carson 1989, 128). In spite of these measurement problems, there seems to be a persistent correlation between these kinds of verbal expressions and actual purchasing behavior (Banks 1950; Gabor 1985; Green and Tull 1978; Udell 1965). Moreover, experimental evidence suggests that people tend to report essentially the same willingness to pay regardless of whether they are required to back up their responses with cash payment (Bohm 1972; Dickie, Fisher, and Gerking 1987).

In order to satisfy both sides of this dispute, I examine actual spending behavior, as well as verbal expressions of willingness to pay. Interestingly enough, the distinction between economic and political decisions is about as clear for cash transactions as it is for verbally expressed willingness to pay. This is not to deny the potential differences between payment and willingness to pay. The point is, rather, that analyses of both dependent variables support the theoretical argument.

Price Elasticity

By *price elasticity*, I mean the rate at which willingness to pay declines as price increases. Placing asking price on the x-axis and the proportion of people willing to pay on the y-axis (Figure 1), one may draw the distinction between *inelastic demand* (the near-horizontal line) and *elastic demand* (the near-vertical line). In the former case, large increases in price produce little change in willingness to pay; in the latter, small price increases produce large changes.[2]

I have chosen to draw curves in Figure 1 rather than straight lines for two reasons. First, I wanted the diagram to reflect the fact that willingness to pay a given price is a dichotomous dependent variable. Since probabilities range between zero and one, it is conventional to conceive of the dependent variable in logistic terms, wherein each unit along the y-axis represents a one-unit change in the log-odds of willingness to pay (Maddala 1988).[3]

Second, intuition suggests that the independent variable (price measured in dollars) will not bear a linear relationship to the dependent variable. To measure price in dollars implies that a $1 change in price has the same impact on willingness to pay regardless of whether this change is from $1 to $2 or from $101 to $102. Previous work on price perceptions and historical patterns of demand shows this assumption to be implausible (Cameron and Huppert 1989; Nicholson 1984, 179–80). This research, as well as the data I shall examine, indicates that the fit between actual and predicted willingness to pay is vastly improved when the natural log of price is used in place of the absolute dollar value. My analyses therefore assume that the log-odds of willingness to pay is a linear function of the log of price. Had I redrawn Figure 1 with logits on the y-axis and the log of price on the x-axis, the two curves would have been straight lines.

How is an analysis of this sort constructed? By way of illustration, suppose n people are asked

1. Would you be willing to pay $10 for a bundle of good X?

Those who respond *no* are then asked

2. Would you be willing to pay $5 for a bundle of good X?[4]

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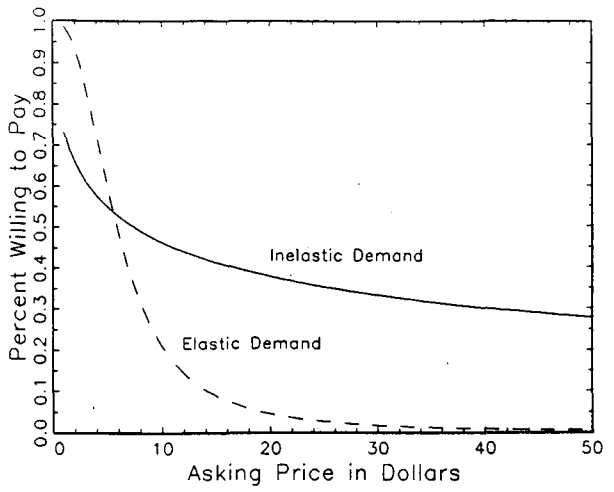
Figure 1.

Elastic Versus Inelastic Willingness To Pay



The frequency distribution of the responses, where *A*, *B*, and *C* are the numbers of people responding in different ways, is

yes to question 1 (willing to pay $10)	A
yes to question 2 (willing to pay $5 but not $10)	B
no to question 2 (unwilling to pay $5)	C
Total	*n*

A cross tabulation of price with willingness to pay would look like this:

Willingness to pay	$5	$10
Willing	$A+B$	A
Unwilling	C	$B+C$

Thus, the log-odds ratio is

$$\ln\left[\frac{A \cdot C}{(A+B)\cdot(B+C)}\right]. \qquad (1)$$

Equation 1 has three implications. First, the slope is undefined if either *A* or *C* equals zero. In other words, if the asking prices are too high or too low, it will be impossible to determine the elasticity.[5] Second, if *B* is zero, the slope estimate will be zero. This, too, makes intuitive sense; for if no one settles for an intermediate price, price lacks causal power.[6] As a practical matter, it is important for the survey researcher to select a range of prices that will produce at least some degree of variability in willingness to pay. Finally, it is apparent that for a given *A* and *C*, the slope estimate decreases (becomes more elastic) as *B* increases. Thus, the more people who settle at an intermediate price (ceteris paribus), the greater the price elasticity.

In order to calculate the price elasticity for good X, one divides the log-odds ratio by the change in the natural log of price, in this case .69.[7] Provided the log-odds ratio is not undefined, this elasticity represents the change in the log-odds of willingness to pay that results from a one-unit change in the log of the asking price. Or, to state the result more cautiously, the estimate reflects the best guess of the price elasticity within the range of prices bounded by *A* and *C*.

Three other methodological caveats are in order. For this analysis to be meaningful, respondents cannot be exempt from the asking price. The fact that between 80% and 87% of nonsmokers favor raising cigarette taxes 5, 10, or 20 cents says little about willingness to *pay* (Green and Gerken 1989). More relevant would be the views of smokers, who are subject to the tax. Second, it is important that the distribution of willingness to pay not be truncated or censored by the use of "filter questions" that are themselves measures of willingness to pay. For example, a 1976 General Electric Quarterly Survey asked respondents whether they would be likely to purchase an electric car and if so, how much they would be willing to pay. This procedure leaves unmeasured the willingness to pay of those not likely to buy. It would be hasty to impute to them a willingness to pay of zero, since they may have had in mind an anticipated purchase price when answering the filter question. To discard these observations would almost certainly lead to biased estimates (Maddala 1988). Therefore, I have excluded a handful of survey items that were preceded by filter questions of this kind (see end of Appendix B).

Finally, in order to facilitate a comparison of demand for public and private goods, it is best to minimize extraneous substitution effects. Appliance stores, for example, feature many comparable brands of consumer durables, so that if one brand goes up in price, people will switch to others (Abrams 1964; Jones 1975). In this situation, price elasticity for a particular brand is greater than the elasticity for the product class (e.g., refrigerators) as a whole. Thus, when gathering data on consumer preferences for private goods, I have ignored the literature on market share and restricted my attention to cases in which people are asked whether they would pay for a type of product.

DATA AND MEASURES

The data analyzed in this paper are drawn from three sources: surveys, experiments, and United Way fund-raising records.[8] In each case, people are asked whether they would be willing to pay for a particular good, be it hardcover books, shelter for the homeless, or commercial-free radio.[9] The survey data were obtained from on-line searches of the Harris, Roper, and Inter-University Consortium for Political and Social Research question text data bases and from manual searches of local, state, and national polls archived at the Roper Center. Experimental data were gleaned from secondary sources. United Way records

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were made available to me by the payroll departments of three large firms in the greater New Haven area.

Over the past 50 years, pollsters and market researchers have devised several ways to offer people different prices. One is to divide the sample into random subgroups and pose a different price to each subgroup. Another approach is to present all respondents with a single price and then ask a follow-up question at a higher or lower price, as in the good X example. Another is to present respondents with a range of different prices simultaneously and ask which is the highest price they are willing to pay. Finally, one may simply ask respondents how much they are willing to pay, without offering any response categories.

Although equivalent in principle, the psychological effects of each question format are likely to differ. When two randomly assigned groups confront different prices, the effects of price tend to be weaker than when the same people face a series of prices (Ehrenberg and Mills 1990; Kosenko 1989; Monroe 1977; Petroshius and Monroe 1987). In the former case, subjects receive no initial price with which to contrast subsequent prices; and the difference between $20 and $25 may fail to produce variation in willingness to pay. The effect of asking respondents to name their prices probably falls in between these two formats. As long as there is variation in the prices named, willingness to pay will fall with price; there is no such guarantee that this will happen when groups are randomly assigned a single price. On the other hand, in the absence of structured response options, responses may become less reliable, and therefore less price-elastic.

One also needs to guard against framing effects that may distort the results. When respondents are asked a long battery of questions concerning their willingness to pay for various goods, they gradually become more thrifty. Thus, willingness to pay becomes confounded with the extent of one's prior expressions of willingness. Similarly, how people react to prices may be conditioned by the prices they received earlier for previous goods. For these reasons, two cases have been dropped from the analysis.[10]

In sum, various methodological considerations led us to restrict the analysis to just 45 items, covering 38 different kinds of goods. These survey questions are described in Appendix B. Despite my selectiveness, this data set remains methodologically heterogeneous. For this reason, I shall now evaluate the robustness of elasticity estimates across time, question format, and question sequence.

DATA ANALYSIS

The Robustness of the Elasticity Estimates

Before taking up the question of whether political goods are less price-elastic than economic goods, one must first ask whether it is meaningful to speak of enduring "parameters" that represent the price elasticity of willingness to pay for a given public or private good. After all, if the estimated price elasticity for a particular good varies substantially from one setting to the next, the utility of studying patterns in the public's willingness to pay is called into question. On the other hand, if parameter estimates display stability over time, then a minimum condition for proceeding with the analysis has been met.

Three sets of questions are available for across-time comparison.[11] Californians in 1988 and 1989 were exposed to virtually identical questions about paying more in taxes to provide shelter for the homeless. Willingness to pay proved to be significantly greater in 1988 than in 1989 ($p < .05$); yet the elasticities are quite similar across the two samples (−.27 vs. −.35), and the difference between them is not significantly different from zero ($p > .05$). Michigan respondents were asked in 1972 and 1974 about their willingness to pay more in state and local taxes for law enforcement. The only variation in question wording was that 1972 respondents were informed that average per capita expenditures in this area were $36, whereas in 1974 this amount was changed to $45 (see Appendix B). Although respondents were significantly more willing to pay higher taxes in 1974 than in 1972 ($p < .05$), the elasticities (−.65 and −.54) are not statistically distinguishable from one another ($p > .05$). Finally, four national surveys between 1973 and 1987 asked Americans whether they would be willing to pay more in taxes in order to fund artistic and cultural activities. Question wording and order varied somewhat; and again, the mean level of public support changed significantly from one survey to the next. Yet the four elasticities are almost identical (−.53, −.46, −.52, −.54). Pooling the data and comparing constrained and unconstrained likelihoods shows that the small differences between elasticities may be attributed to sampling variability ($p > .05$).

Elasticity estimates also seem to be quite robust across variations in question wording at any given point in time. Kahneman (1986) conducted an experiment in which Ontario respondents were asked about their willingness to pay higher taxes to fight water pollution and preserve fishing in the region. The definition of the region, however, varied from one experimental group to the next. Some respondents were asked about their willingness to pay to preserve fishing in Muskoka; others, Haliburton; others, Ontario as a whole. The three elasticities are quite similar (−1.39, −1.50, −1.47; $p > .05$), despite the fact that the cleanup of all of Ontario was 5–15% more popular than the other cleanup proposals.

These results suggest that the price elasticities associated with particular goods have an enduring quality; for even as goods grow or diminish in attractiveness, their elasticities remain stable over time and amid variations in question wording. Adding to the strength of these results is a recent study of the British public's willingness to pay for British Broadcasting Corporation channel 1 television on a sub-

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scription basis, indicating that elasticity estimates also tend to be relatively insensitive to the form in which prices are stated. Respondents to different surveys conducted during 1989 and 1990 were offered prices ranging from £44 to £240 per year on an annual or monthly basis, depending on the survey (Ehrenberg and Mills 1990, 13). These data yield elasticities of −1.70 and −1.76 ($p > .05$) for annual and monthly question formats, respectively. In essence, the shape of the willingness-to-pay distribution remains unchanged across equivalent price ranges expressed in different units, despite the fact that the mean level of willingness to pay is significantly higher ($p < .05$) when prices are asked in terms of monthly rates.

Finally, one may gauge the robustness of price elasticity estimates across variations in question sequence. The results concerning cultural activities give a preliminary indication that variations in question order do not alter the results dramatically. A more rigorous test is made possible by a 1963 National Opinion Research Center split-ballot experiment in which people were asked whether they would be willing to pay for a household device designed "to alert the population in case the United States were to be attacked by an enemy." Respondents were randomly assigned to one of four initial asking prices ranging from $5 to $20. After responding to the first price, subjects were asked to respond to the three other prices; thus, some respondents received the prices in ascending order, others in descending order. As one might expect, random assignment of price generates an elasticity estimate that is somewhat weaker than that produced by an ordered series of price offers (−.76 vs. −1.00; $p < .05$).[12] But (as I shall demonstrate) this range of variation is dwarfed by the variation between the elasticities of consumer goods and public goods. In other words, question format may alter the estimates somewhat, but not enough to affect the overall pattern of results.

The Consistency of Price Elasticities across Subgroups

My procedure for evaluating price elasticity presupposes that the influence of price is constant across all the individuals within a sample. Ignored, therefore, are variations in cost sensitivity that might arise because some people are strapped for cash or committed to a political ideology that stresses fiscal restraint. Before turning to the comparison of public and private goods, I take note of the fact that elasticity estimates seem not to vary markedly from one subgroup to another.

Consider, for example, willingness to pay to support the arts and cultural activities in 1973. As one would expect, the frequency with which people attend artistic performances is a strong predictor of willingness to pay: 62% of those Farrell characterizes as "frequent cultural attenders" were willing to pay $50 more in taxes, as opposed to 17% of "cultural nonattenders" (1975, pp. 6, 112). But despite the sharp difference in *mean* willingness to pay, the elasticities for the two groups were −.54 and −.48, respectively.[13] Very similar results obtain when one stratifies the sample by income, age, education, region, or race. For example, although willingness to pay increases sharply with income, elasticities for those making less than $5 thousand, $5–10 thousand, $10–15 thousand, and over $15 thousand turn out to be quite close (−.49, −.48, −.51, and −.57, respectively) and not significantly different from one another ($p > .05$).

These results are corroborated by other investigations of this sort. In an earlier work, I analyzed willingness to pay for (1) shelters for California's homeless, (2) an expanded subway system in Los Angeles, and (3) reductions in water pollution. In each case I found that elasticity estimates varied little across categories of income, ideological self-designation, and region (Green 1988). Similarly, Ehrenberg and Mills (1990) found that the elasticity of willingness to pay for subscription television varied little among groupings of social class and amount of television viewing; results presented by Cameron and Huppert (1989) indicate that the dispersion of anglers' willingness to pay to support fish hatcheries in the San Francisco Bay did not change when controls were added for income, number of fishing trips in the past year, and boat ownership; and Adam's (1958) study of willingness to pay for clothing and durable goods turned up little variation in elasticity across income and occupational strata. In short, while groups vary with respect to their mean level of willingness to pay, the rate at which willingness to pay changes with price is remarkably consistent across groups. In light of these findings, I feel confident that the strategy of treating samples in an undifferentiated way does not distort my conclusions.

THE CONTRAST BETWEEN POLITICAL AND ECONOMIC GOODS

Are public/political goods more price-elastic than private/economic goods? Let us begin by developing standards by which to distinguish between public and private goods. Four criteria suggest themselves:

1. *Excludability versus equal entitlement.* Who is entitled to use the good? In the field of economics, public goods are defined as those which, once produced, may be consumed by anyone. National defense and wildlife preserves, for example, represent public goods because no citizen may be prevented from consuming them (Nicholson 1984).[14]
2. *Individual versus collective payment.* Is payment to be an individual or collective act? One may distinguish between purchases that are made in concert with others (e.g., through taxes or contributions) and transactions in which the purchase results from an individual's action (e.g., an exchange transaction). The former brings to mind an additional psychologi-

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TABLE 1

Classification of Goods along the Public–Private Continuum

Private. Exclusive and personal consumption and individual payment; not associated with the public welfare: toilet soap, subscription television (BBC channel 1), lottery tickets, pay-per-view baseball television, newly introduced communications product, refrigerators, nylon stockings, hardcover books, newspapers, housing, and health insurance.

Semiprivate. Exclusive and personal consumption and individual payment; associated with the public welfare. Exclusive and personal consumption but collective payment; not associated with the public welfare. Or personal consumption and individual payment but semiexcludable. Examples are safety caps on prescription bottles, Swedish television comedy show, and recreational fishing.

Mixed. Exclusive and personal consumption of the product to be taxed but nonexclusive personal consumption of the good funded by the tax; collective payment imposed only on those who consume the product; associated with the public welfare:

cigarette tax designed to reduce a budget deficit
gasoline tax designed to build roads
gasoline tax to reduce dependence on foreign oil
sales tax to fund World War II war effort
civil defense alarm
increase in monthly electricity bills for pollution abatement
increased taxes and consumer prices for clean drinking water
increased gasoline prices to sustain Iraq oil embargo
anglers' support for fish hatcheries and habitat restoration

Semipublic. Nonexclusive consumption and collective payment; associated with the public welfare. However, the good may be consumed personally:

preserving fishing in Ontario lakes
fighting pollution in the state's fresh water lakes
artistic and cultural activities
building a subway to combat pollution and traffic congestion
commercial-free radio
planting trees in a nearby park
fighting crime in one's state
cleaning the environment in one's state
increased family expenses to improve the environment

Public. Nonexclusive consumption and collective payment; associated with the public welfare; unlikely to be consumed personally: shelter for the homeless, reducing the federal deficit, cleaning up the environment, clean air, clean water, and saving endangered species.

cal consideration: To what extent should the collectivity take action on behalf of this objective?

3. *Personal versus social consumption.* Is the good in question of personal use to the person being asked to pay? Or does it primarily meet social needs that are more or less independent of the personal needs of the purchaser? When one pays to supply coffee in one's office or clean up air pollution in one's locality, the benefits of doing so are likely to be felt personally. At the opposite end of the continuum are donations designed to combat starvation in a distant country, a program that is likely to have no tangible impact on the donor.[15]

4. *Public welfare goods versus non–public welfare goods.* What ends are served by the good in question? Even goods that are consumed personally, such as safety caps on prescription bottles, may serve a broader social purpose.

Using these criteria, I classified the 38 types of goods along a five-point scale (See Table 1). On one end of the continuum are goods that may be classified as private by all four criteria; on the other, those which are wholly public.[16]

Is there a relationship between the public–private continuum and price elasticity? Price elasticities were calculated for each good. Where duplicate questions existed, the elasticity estimates were derived from analyses in which the data sets were pooled. Figure 2 presents the elasticity estimates in the form of a scatterplot. The strong upward tendency of the points (Spearman's $\rho = .74$) indicates that the elasticity of private/economic goods is considerably greater than the elasticity of public/political goods.[17]

SOME STATISTICAL REFINEMENTS

Although it is gratifying to see the contrast between public and private goods emerge so clearly, the analysis requires some refinement, since the elasticities for certain public goods are so weak as to be anomalous. Take, for example, a good such as shelter for the homeless, which has an elasticity of $-.31$ and, in 1988, an intercept of 2.3. The hypothesized linear relationship between the log-odds of willingness to pay and the log of price fits the data quite well within the \$5–\$50 range of prices presented in the survey ($\chi^2_{(2)} = 1.95$, $p = .38$); yet if one were to extrapolate

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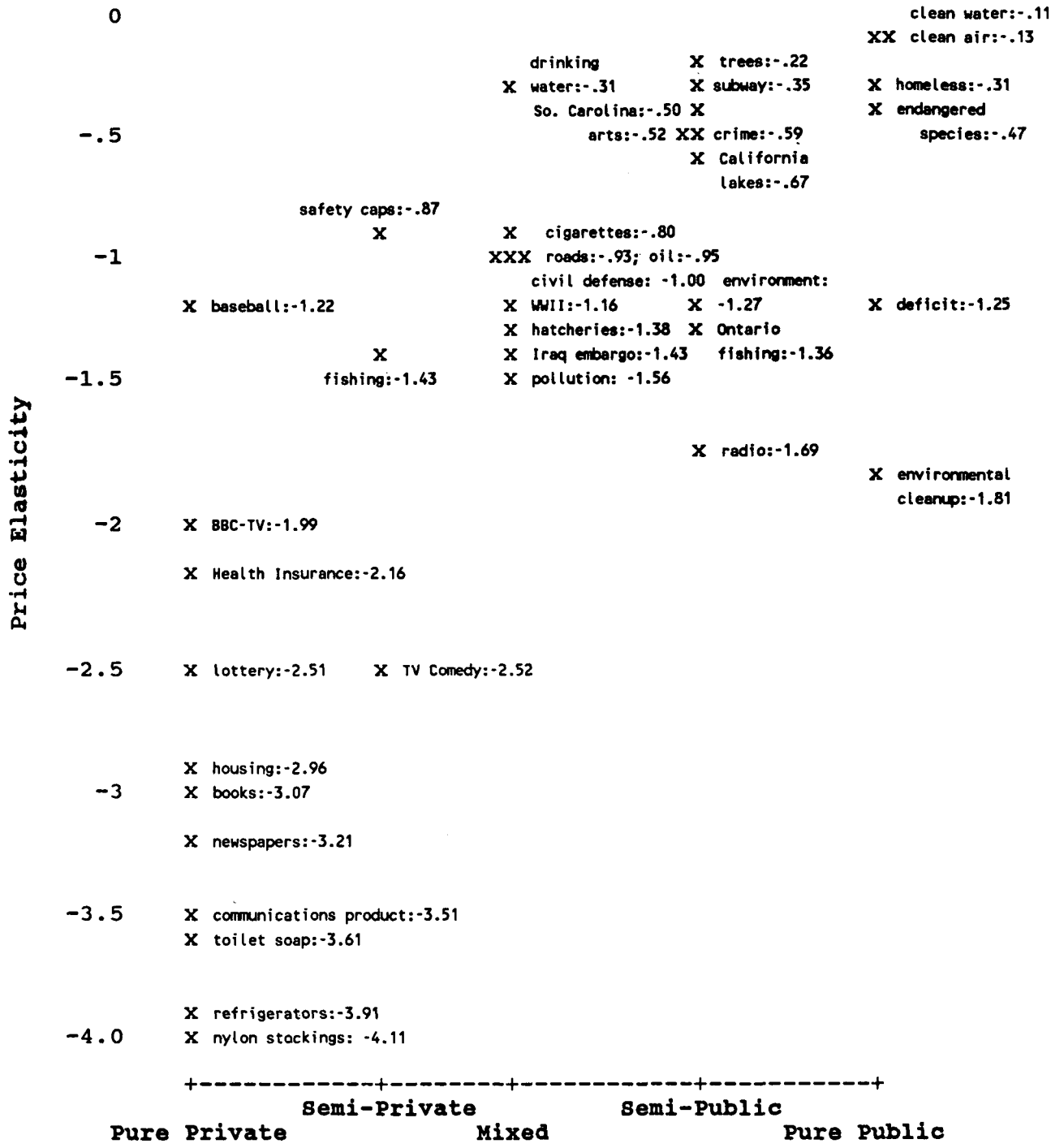
Figure 2.

Scatterplot of Price Elasticity by Private/Public Continuum



beyond the range of the data, one would draw the implausible conclusion that 54% of the California public were willing to pay a thousand dollars more in taxes.

This kind of anomaly would seem to suggest that elasticities increase at higher price ranges. To test this proposition, I took a closer look at three goods in my data set that were offered at widely varying price ranges: British Broadcasting Corporation subscriptions (a private good), anglers' support for fish hatcheries (a mixed good), and efforts to preserve endangered species (a public good). In each instance, there is strong evidence of acceleration of price elasticity. As shown in Table 2, a significant improvement in fit occurs when one models the process of acceleration by including a quadratic logged price term.[18] Inter-

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TABLE 2

The Effect of Price on Willingness to Pay, Accounting for the Acceleration in Price Elasticity

VARIABLE	LOGIT ESTIMATE	STANDARD ERROR
Preserving endangered species[a]		
ln(Price)	.479	.088
ln(Price)^2	−.190	.018
Intercept	−.327	.044
Anglers' support for fish hatcheries[b]		
ln(Price)	.101	.298
ln(Price)^2	−.210	.047
Intercept	1.865	.475
Subscriptions to BBC channel 1[c]		
ln(Price)	.136	.794
ln(Price)^2	−.244	.092
Intercept1	4.190	1.730
Intercept2	4.764	1.720

Note: Estimates were obtained by maximum likelihood. ln(Price) represents the natural log of the asking price. Intercept1 refers to the intercept for cases gathered using a maximum price response format; Intercept2 is the corresponding intercept for cases gathered using a Gabor–Granger method (see Ehrenhart and Mills 1990). L_1 represents the log-likelihood for the quadratic model; L_0, the model without the quadratic term. df = 1, $p < .05$.

[a] $-2\ln\left(\frac{L_1}{L_0}\right) = 231.9.$

[b] $-2\ln\left(\frac{L_1}{L_0}\right) = 20.8.$

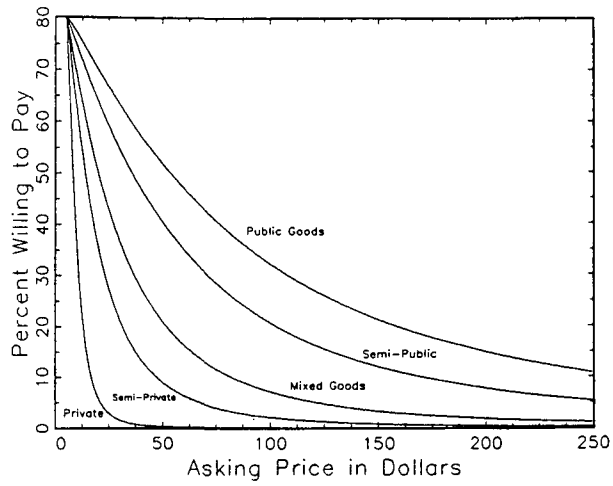
[c] $-2\ln\left(\frac{L_1}{L_0}\right) = 6.3.$

estingly, the rate of acceleration turns out to be quite similar for each of the goods. If anything, the estimates suggest that the rate of increase in elasticity is faster for goods that are private in character (nonsignificant, $p > .05$). The conclusions to be drawn from this digression are threefold. First, once the rate of increase in elasticity is taken into account, anomalies such as the one mentioned disappear. If willingness to pay to shelter the homeless is assumed to follow a trajectory similar to other high-priced goods, the predicted level of support for a thousand-dollar tax would be a more plausible 7%. Second, the notion that higher prices elicit more price-elastic responses accounts for two of the most troublesome outliers in Figure 2, deficit reduction and cleaning up the environment (both relatively price-elastic but classified as pure public goods). Had the prices offered for these goods varied between $10 and $50, rather than between $100 and $500, the elasticity estimates would probably have fallen within the range of −.30 to −.60. Finally, while the elasticity estimates in Figure 2 are to some extent conditional on the range of asking prices presented to respondents, the contrast between public and private goods persists when the rate of acceleration is taken into account.

Figure 3 illustrates the differences among the five

Figure 3.

Willingness-To-Pay Curves for the Median Good in Each of the Five Categories of the Public/Private Continuum



categories of public and private goods. In order to facilitate comparison, I have assumed that 80% of the public is willing to pay $5 for each type of good. Each line depicts a trajectory of willingness to pay, assuming the median elasticity for private (−3.07), semiprivate (−1.40), mixed (−1.25), semipublic (−.59), and public goods (−.39). In order to take into account the acceleration in elasticity, I have assumed a quadratic effect of −.20 for each type of good.[19] Notice that by the time the asking price reaches $50, the five lines have diverged considerably: fewer than 1% of the public are willing to pay for the private good, whereas a majority are willing to pay for the public good. When price reaches $250, willingness to pay for the hypothetical private good is all but nonexistent; whereas almost 15% are still willing to pay for the public good. In short, when price increases by an order of magnitude, the result is a decline in the demand for both public and private goods; but the rate of decline is much more precipitous for the latter.

IS THE PRICE–QUALITY RELATIONSHIP A CONFOUNDING FACTOR?

So far, I have considered just one kind of price effect: the negative influence price exerts on demand through the operation of budget constraints. There is, however, a countervailing price effect, which stems from the fact that people sometimes infer a relationship between price and quality (Lichtenstein and Burton 1989). Consumers, for example, may suppose expensive wristwatches to be higher in quality; voters may infer that a $50 tax will rescue more homeless people than a $5 tax. Because this type of inference

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tends to reduce the price elasticity of willingness to pay, one must guard against the possibility that the public goods in my sample are more prone to price–quality imputations than the private goods.

Experimental evidence drawn from the contingent valuation literature lends little support to the idea that the perceived link between price and quality is especially strong where public goods are concerned. Brookshire and Coursey (1987) asked one set of respondents how much they would be willing to pay to finance a precisely delineated plan to plant 25 additional trees in a neighboring park. Another set of respondents were asked about the same plan, only with the understanding that the more money raised, the more trees planted. Thus, in the latter case higher expenditures seemed to translate into a higher quality public good, whereas in the former case respondents were offered a "fixed bundle" of public goods. Yet my reanalysis of these data indicates that the price elasticity is not significantly different across the two conditions ($p > .05$). Moreover, willingness to pay for a fixed bundle of trees is *less* responsive to price than most other public goods listed in Figure 2.[20]

Another strike against the price–quality hypothesis is that the effects it ascribes to quality inferences are implausibly large. Consider, for example, what the distribution of willingness to pay to shelter the homeless would have looked like if public goods were in fact as price-elastic as private goods. In 1988, approximately 84% of the California public supported a $5 tax to shelter the homeless; 74% favored a $50 tax. If the elasticity were −1.5 (weak by the standards of private goods), then only 14% of the public should have supported a $50 tax. Clearly, the difference between 74% support and 14% support cannot be ascribed to the greater effectiveness of a program funded by a $50 tax; for (as I noted in discussing Kahneman's [1986] experiment concerning Ontario water pollution) willingness to pay is relatively insensitive to the scope of a proposed program (see also Brookshire and Coursey 1987; Kahneman and Knetch n.d.).

The final word on the influence of price–quality inferences awaits further experimentation. For the moment, I think it unlikely that imputations of quality account for the sizable differences between public and private goods; for if the scope or character of a public program were crucial, then one would expect to see a greater contrast between the elasticities associated with large programs funded by general tax increases (where an increase in price may well correspond to increased provision of a public good) and those that rely on voluntary contributions (where one's own expenditure has no real impact on the dimensions of a public good). I shall demonstrate that this contrast is all but absent.

ATTITUDES VERSUS BEHAVIOR

As I have noted, a certain amount of skepticism surrounds the use of surveys to measure willingness to pay. For this reason, I now take up the question of how the price elasticity of willingness to pay changes when money actually changes hands. A preliminary answer is that the difference is not great. Bohm's (1972) data on willingness to pay to see a comedy special, for instance, show no significant difference ($p > .05$) between the elasticities of verbal expressions and actual payments. Nor are lottery tickets (for which people actually paid cash; Knetch and Sinden 1984) distinctive in terms of price elasticity (see Figure 2). In general, experiments comparing actual payments and verbal expressions of willingness to pay for private and semiprivate goods have drawn the somewhat surprising conclusion that the two have very similar distributions (Dickie, Fisher, and Gerking 1987; Heberlein and Bishop 1986).

Much the same argument may be made for public goods. Data on United Way contributions by employees of three large corporations in the greater New Haven area were obtained.[21] The United Way is an umbrella organization that funds a variety of charitable organizations and programs. It is a pure public good, since payment is collective, consumption is nonexcludable, and few if any contributors benefit directly from its services. Moreover, because payment takes the form of contributions rather than taxes, those who are willing to pay do not have coercive power over others. This is significant in light of the concern that price may be regarded as an indicator of quality. Here, paying more does not mean that the United Way budget will expand proportionally. The United Way services are essentially constant across different prices to the contributor.

Price Elasticity of Contributions

What can the distribution of United Way contributions tell us about the relationship between cost and willingness to pay? The three firms surveyed vary considerably in the strength of their United Way campaigns. In Firm A, 77% of all employees contributed at least $10 to the United Way in 1989. The corresponding figures for Firm B and C were 58% and 24%, respectively. Despite these differences, the price elasticities of United Way contributions turn out to be similar across firms (see Table 3). When we pool the three firms together, we find that the elasticity of contributions is roughly −.3 when the "asking price" is $15, −1.4 when the price is $100, and −2.3 when the price is $500.[22] There is, in other words, a close match between the results obtained from the United Way data and the earlier conclusions based on verbally expressed willingness to pay for pure public goods.

Admittedly, this brief examination of charitable contributions to the United Way cannot be construed as a representative sample of contributing behavior, particularly given the diversity of religious, political, and cultural institutions that raise money. These findings, however, are consistent with studies of donations to the Humane Society (Fraser, Hite, and Sauer 1988), the American Cancer Society (Cialdini

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TABLE 3

The Price Elasticity of United Way Contributions

FIRMS	ESTIMATE	STANDARD ERROR
Firm A[a]		
ln(Price)	1.128	.171
ln(Price)^2	−.279	.023
Intercept	.097	.294
Firm B[b]		
ln(Price)	1.232	.501
ln(Price)^2	−.288	.070
Intercept	−.980	.809
Firm C[c]		
ln(Price)	1.354	.430
ln(Price)^2	−.278	.060
Intercept	−2.617	.682
All Firms[d]		
ln(Price)	1.215	.151
ln(Price)^2	−.285	.020
Firm A	−.126	.257
Firm B	−.952	.268
Firm C	−2.212	.256

Note: Estimates were obtained by maximum likelihood. ln(Price) represents the natural log of the contribution. ln(Price)^2 is the square of this variable. Other variables are the intercepts for each of the different corporations. Note that due to the large number of cases, the elasticities of the three firms differ by a statistically significant degree ($p < .05$), although substantively, the trajectories are quite similar.
[a]n = 1,615; log-likelihood = −2,162.1.
[b]n = 342; log-likelihood = −406.1.
[c]n = 700; log-likelihood = −584.6.
[d]n = 2,657; log-likelihood = −3,161.1.

and Schroeder 1976), and political candidates in Ohio (Snyder 1991). These findings are also consistent with preliminary results from my own investigation of contributions to religious organizations, community programs, cultural institutions, and political campaigns. The only discrepant case I have encountered is the pattern of contributions to a local natural history museum, which displayed an elasticity similar to a semiprivate good. But here the money was raised through a membership drive, in which prospective donors were offered a package of benefits (free admission, a newsletter, etc.) in return for their contributions. Apparently, by marketing the consumption-related benefits associated with the museum, the fund-raising campaign increased the price-elasticity of willingness to pay; for its other campaigns, which did not solicit museum memberships, received more widely varying donations. Although the conclusions I draw from this limited sample of contributing behavior are necessarily tentative, the pattern of results is consistent with findings based on survey data.

DISCUSSION

One of the most hallowed principles in microeconomics is the idea that "a rise in price reduces quantity demanded, be it a rise in the market price of eggs, a rise in the 'shadow' price of children, or a rise in the office waiting time for physicians" (Becker 1976, 6–7). The findings presented here do not dispute this claim, for even pure public goods turn out to be price-elastic. But the evidence does suggest the corollary that goods that are evaluated solely in terms of personal consumption are particularly sensitive to price. Microeconomic reasoning is not inapplicable to mass political preferences; but neither is it especially powerful. Unlike the consumer, whose willingness to pay is quickly extinguished by price increases, the voter tends to be much less sensitive to considerations of personal cost.

Consider, for example, the limited influence of cost on public opinion toward redistributive social welfare policies. As I have noted, whether the price is $5 or $50, a sizable majority of Californians support a program designed to shelter the homeless. Analogous results obtain when respondents' social characteristics serve as proxy measures of price. The unemployed have more to gain from a government policy of full employment than those for whom unemployment is an unlikely prospect; moreover, the unemployed have less to lose, since the tax burden would fall more heavily on people with jobs. Yet, cross-sectional surveys of opinion show that the views of the employed and unemployed are not markedly different (Schlozman and Verba 1979); and panel studies indicate that those who experience unemployment or reduced hours during a recession do not become more supportive of policies designed to alleviate unemployment (Sears et al. 1980). Similarly, the medically indigent would benefit from a system of national health insurance at little cost to themselves; their costs would be borne by others. But despite the fact that the indigent and nonindigent would expect to pay different prices for national health insurance (to say nothing of differential benefits), the former are only mildly more supportive of the policy (Green 1988; Sears et al. 1980). In short, the costs of social welfare policies may differ from one social group to the next; but price is seldom a strong correlate of policy support.

If anything, data on the public's willingness to pay provide more evidence of self-interest than many previous studies of mass political attitudes. Considerations of cost, for example, determine whether support for expanding government funding for the arts runs below 45% or upwards of 65%. Not so for opinions towards affirmative action, social security, aid to education, or women's rights (see Citrin and Green 1990; Sears and Funk 1990), which show little covariation with personal costs or benefits. Two factors may account for the apparent contrast. Unlike previous research on the role of self-interest in public opinion, I have analyzed survey questions that state the price of policy support in an explicit fashion. Respondents did not have to figure out for themselves whether their jobs would be threatened by a state budget cut (Sears and Citrin 1985), whether their children would be affected by a desegregation ruling (McConahay 1982), or whether their family might experience a tragic loss at the hands of war

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(Lau, Brown, and Sears 1978). Moreover, the fact that price was explicitly mentioned in the survey question meant that respondents were primed to think about considerations of cost—something that survey respondents do not ordinarily do when evaluating policy questions (Green 1988; Lau and Sears 1981; Sears and Lau 1983).[23]

Yet even when costs are made salient and stated in an explicit fashion, the price elasticity of public goods lags far behind the elasticity of commercial goods. What accounts for the contrast between political and economic goods? One explanation might be that people lack a clear sense of how much public goods should cost or, similarly, how much public good can be purchased for a given price (Cox 1986; Sears and Citrin 1985). Uncertainty about the appropriate price of a political good increases the variability in willingness to pay, thereby weakening the price elasticity. The implication is that if citizens had more experience as purchasing agents for public goods, the price elasticity of willingness to pay would increase. One might add that this hypothesis would also predict that economic goods with which the public is unfamiliar will tend to have lower price elasticities. Perhaps this is why a 1959 question concerning pay-per-view cable television produced the weakest price elasticity of the 11 private consumption goods.

Experimental evidence on the role of information shows that intensive exposure to going prices for consumption goods over an extended period of time produces more price-elastic preferences (Diamond and Campbell 1989). Comparable experiments have not been undertaken using nonmarket goods. Interestingly, however, the experiments that have been performed indicate that imparting information about public goods seldom alters the distribution of willingness to pay (Cummings, Brookshire, and Schulze 1986), a conclusion that also emerges from the data presented here. Michigan respondents, for example, were told the per capita cost of law enforcement before being asked their willingness to pay for more; yet the elasticity was just −.59. Information about per capita expenditures on the arts prefaced a series of willingness-to-pay questions; yet the elasticities hovered around −.52. And although Brookshire and Coursey (1987) took pains to inform respondents about the costs and benefits of planting trees in a nearby park, the elasticity was just −.22. While this line of explanation is intuitively appealing, the influence of price-related information on the elasticity of public goods has so far eluded detection.

An alternative argument is that asking price is simply one of several considerations that come to mind when people make decisions (Quandt 1956). Price may, for example, compete with feelings of sympathy or ethical commitments for the attention of the decisionmaker (Etzioni 1988; Sen 1977). Deciding whether to purchase cable television brings to bear relatively few considerations that go beyond price and consumption utility. By contrast, deciding whether to support a tax increase to shelter the homeless involves considerations such as sympathy for the destitute, the appropriate role of government, and perhaps certain racial attitudes as well. As a result, the cognitive weight assigned to price is reduced,[24] while variance stemming from diversity in political predispositions is injected into the distribution of willingness to pay, thereby weakening price elasticity.

The contrast between political and economic goods, according to this argument, hinges on the nature of what is to be decided. Conceivably, certain economic decisions, such as whether to purchase grapes during a farm workers strike, could involve normative considerations that distract attention away from price. By the same token, some political decisions may vary in terms of the potency and number of nonprice considerations that they raise. Over a wide variety of goods, however, this hypothesis suggests that price tends to receive less attention in the ballot booth than in the shopping mall.

A telling indication of how the absence of an ideological or moral dimension to a monetary choice affects the nature of willingness to pay may be found in Lynn's (1990) study of a restaurant that allowed its customers to choose the prices they paid for dinner entrées. At the conclusion of the meal, each customer was presented with a bill that listed four prices: a minimum price and prices 16%, 28%, and 38% higher than the minimum price. While Lynn makes much of the fact that almost half of the customers paid more than the minimum price they were offered, the results may be interpreted somewhat differently. In contrast to the relatively cost-insensitive pattern of contributions to the United Way, donations to the restaurant in Lynn's study displayed an elasticity of −21.8 (SE = 1.8), since just 2% of the customers voluntarily doled out the maximum asking price. Evidently, Adam Smith was right when he noted that economic enterprise could not survive on benevolence alone; for in absence of significant ethical or ideological considerations to call attention away from considerations of cost and utility, the tendency to economize asserts itself.

One further explanation for the apparent contrast between public and private goods has to do with the availability of substitutes. The market economy, one could argue, provides consumers with an array of different products to accommodate a given taste. Substitutes have the effect of winnowing the upper end of the willingness-to-pay distribution; for if the price of a television subscription becomes too high, people may satisfy their thirst for entertainment by watching movies or reading. Less winnowing occurs where public goods are concerned, one might argue, because nonmarket goods are less readily substitutable. What alternatives are available, for example, to those who wish to clean up air pollution in the Grand Canyon but discover that the price of doing so exceeds their willingness to pay?

At present, the role played by substitution effects remains an open empirical question. The literature on willingness to pay offers little guidance here; and my

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data are rather ambiguous. Looking back at Figure 2, private goods would appear to be more readily substitutable. On the one hand, consumption goods such as refrigerators or new communications products (for which there are few substitutes) have sharp elasticities, while public goods such as subways or the arts (for which there are several potential substitutes) have weak elasticities. In none of the cases, however, were respondents encouraged to consider alternative means by which they might accomplish their public or private objectives.

The three explanations—price awareness, price salience, and availability of substitutes—are complementary. The next step is to assess the extent to which each factor explains the distinctiveness of public demand for nonmarket goods. If the price salience interpretation is correct, it should follow that the distribution of willingness to pay changes depending on whether respondents are primed to consider the instrumental or symbolic attributes of a particular good. If price awareness is crucial, then providing respondents with information about going prices should make preferences more elastic. And if substitution effects are important, priming respondents to consider a particular good against a backdrop of possible alternatives should enhance elasticity. The relative importance of information, symbolism, and substitutes is not only a matter of theoretical interest; it has important practical implications for those who wish to shape public opinion by enhancing or weakening the elasticity of public demand.

POLITICAL IMPLICATIONS OF THE PUBLIC'S WILLINGNESS TO PAY FOR PUBLIC GOODS

The results presented here may be regarded as something of a departure from previous findings concerning public attitudes toward taxes and spending. Since the advent of survey research during the 1930s, scholars have taken note of the "something for nothing" paradox: the fact that the public's deep-seated hostility toward taxes and "big government" coincides with its desire to see increased government spending on many (if not most) public services (Key 1961; Sears and Citrin 1985; Studenski 1939). The data analyzed here suggest that when forced to confront the trade-off between public goods and taxes, the public *is* in many instances willing to pay for increased spending in specific policy areas. Indeed, many of the surveys seem to indicate that the reservoir of willingness to pay for public goods at times runs quite deep.[25]

The survey context, however, tends to be quite different from the political context. Virtually all of the surveys I have encountered try to "sell" the idea of a tax increase to the respondent. Even the less tendentious questions avoid all mention of considerations that might undermine public support (see Appendix B). Actual campaigns, on the other hand, tend to be two-sided affairs; and the potency of negative symbols such as bureaucratic waste, big government, and undeserving beneficiaries is what often determines the fate of tax-related ballot measures.[26] The connection between survey responses and actual support for tax increases becomes more tenuous as one turns from ballot measures to legislative actions. In the survey context, respondents are asked whether they are willing to pay more to fund a particular public good. Very often, the interviewer makes the explicit stipulation that the funds generated will be used solely for the public good in question. While tax measures that feature this kind of one-to-one correspondence between revenues and expenditures arise from time to time in the form of social security taxes or war-related surcharges, many legislative acts, such as the 1990 federal tax increase, raise revenues for a host of public goods threatened by budgetary shortfalls. Under these circumstances, the level of public willingness to pay is contingent on how the public perceives the good that is to be funded through increased taxes. The public might begrudgingly accept a tax increase in return for "deficit reduction"; but tax increases designed to maintain "government services" are unlikely to have widespread appeal. Unlike law enforcement, health care, or environmental protection, nonspecific references to goods provided by government seldom prevail in trade-offs with "higher taxes" (Sears and Citrin 1985).

Finally, it should be noted that political considerations make it risky for public officials to cash in on the public's willingness to pay for public goods. Naturally, tax increases make no friends among citizens who profess unwillingness to pay the amount that is to be exacted from them. But even taxes that seem to fall below the threshold people are willing to pay may provoke electoral reprisal, since willingness to pay need not imply eagerness to pay. This, in combination with the fact that the public often loses track of the connection between revenues and services, means that public officials risk being remembered primarily as the politician who raised taxes. Small wonder that even when elected officials appear to have a clear survey-based mandate to raise taxes for the purpose of improving education, reducing water pollution, or fighting homelessness, their political instincts make them reluctant to do so.

As a practical matter, then, the percentage of the public willing to pay a given price for a collective good is a politically illusory datum. But the depth, or absolute level, of public support has not been my central concern. (Here, I depart markedly from previous research involving the valuation of public and private goods, which focuses almost exclusively on a population's mean willingness to pay.) Instead, the focus has been the *elasticity* of the public's valuation of collective goods, which is arguably more important in shaping the interplay between public opinion and policymaking. The fact that public opinion changes slowly in response to changing costs affords public officials considerable latitude in implementing policies, particularly when the policies are supported by

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strong political attitudes. Enormously expensive undertakings, such as Operation Desert Storm, can be pursued on the assumption that if a majority of the public will swallow a $50 tax hike, a sizable proportion is probably willing to go along with a tax increase of $100.

But if inelastic demand frees the hands of policymakers in certain respects, it ties them in others. In particular, it is difficult for public officials to garner public support for a proposed program by reducing the costs it imposes. According to my estimates, slashing a proposed $100 tax in half tends to boost willingness to pay for a public good by only 5–10%.[27] Unless the supporters of an unpopular public good are willing to scale back their ambitions markedly, they will gravitate toward alternative strategies, such as redefining a good so as to appeal to the ideological or symbolic orientations of the electorate (Edelman 1964).

In conclusion, it may be said that the price elasticity of mass preferences and the nature of public discourse reinforce one another. The weak elasticity of willingness to pay for public goods reflects the extent to which questions of policy draw attention to symbolic or ideological issues. At the same time, the weak elasticity of public goods creates a system of incentives that shunts political discourse away from issues of cost or efficient alternatives, toward symbolic or ideological appeals. A similar reciprocal process seems to operate in the economic realm. The price elasticity of private goods may reflect the relative absence of considerations apart from personal utility; conversely, the potency of price may help make price-based appeals ubiquitous in the marketplace. Thus, not only are economic and political decisions different in character, but the fact that these decisions take place in different environments helps to sustain the schism between the consumer and the consumer's less price-conscious alter ego, the citizen.

APPENDIX A: LIKELIHOOD FUNCTIONS FOR WILLINGNESS-TO-PAY DATA

Maximum likelihood estimation requires one to specify the likelihood function for the observations. For the willingness-to-pay data analyzed here, this likelihood function may take one of two forms, depending on how the price questions are presented. The first likelihood function applies to surveys in which a single price is offered to each of several different subsamples (e.g., the homeless question posed to California respondents in 1989 in which half of the sample was asked whether they would pay $10, half was asked whether they would pay $50). Let w be a random variable representing the maximum amount each person is willing to pay to shelter the homeless. Let $\{p_1, p_2, \ldots, p_k\}$ represent an ordered, nonnegative series of prices to be offered. In this case, $p_1 = \ln(\$10)$ and $p_2 = \ln(\$50)$. Finally, let $I^{no}_{\$10}$ represent the number of people observed who refuse to pay $10; $I^{yes}_{\$10}$ is the number willing to pay $10. A similar notation, $I^{no}_{\$50}$ and $I^{yes}_{\$50}$, applies to the responses of the other half-sample to the asking price of $50.

The likelihood function for the data for an arbitrary probability distribution would be

$$L = \Pr[w < p_1]^{I^{no}_{\$10}} \cdot \Pr[p_1 \le w]^{I^{yes}_{\$10}} \cdot \Pr[w < p_2]^{I^{no}_{\$50}} \cdot \Pr[p_2 \le w]^{I^{yes}_{\$50}}.$$

Hence, the log-likelihood would be

$$\ln L = I^{no}_{\$10} \ln \Pr[w < p_1] + I^{yes}_{\$10} \ln \Pr[p_1 \le w] + I^{no}_{\$50} \ln \Pr[w < p_2] + I^{yes}_{\$50} \ln \Pr[p_2 \le w].$$

If one assumes (as I do throughout) that willingness to pay is distributed log-logistically, the likelihood function for the data is

$$\ln L = I^{no}_{\$10} \ln \left[\frac{e^{-a-bp_1}}{1+e^{-a-bp_1}}\right] + I^{yes}_{\$10} \ln \left[\frac{1}{1+e^{-a-bp_1}}\right] + I^{no}_{\$50} \ln \left[\frac{e^{-a-bp_2}}{1+e^{-a-bp_2}}\right] + I^{yes}_{\$50} \ln \left[\frac{1}{1+e^{-a-bp_2}}\right]. \quad \text{(A.1)}$$

This functional form extends easily to surveys that divide respondents into k different price subgroups. This likelihood function, incidently, is identical to the one used by statistical packages such as SPSS-X when they run "logit." The values of a and b that maximize this likelihood function are maximum likelihood estimates of the intercept and price elasticity, respectively.

The other likelihood function applies to cases in which a series of prices are offered to a single sample. (See the 1973 Harris questions concerning cultural facilities; the sample was offered an initial price of $50, and those who refused were asked whether they would pay $25.) Let $\{I_1, I_2, \ldots, I_{k+1}\}$ represent the number of respondents who fall in the various intervals between and outside the k price thresholds, so that I_1 people refuse to pay p_1, I_2 refuse to pay p_2 but agree to pay p_1, etc. Now the log-likelihood function becomes

$$\ln L = I_1 \ln \Pr[w < p_1] + I_2 \ln \Pr[p_1 < w < p_2] + I_3 \ln \Pr[p_2 \le w].$$

Assuming w_i to be distributed log-logistically:

$$\ln L = I_1 \ln \left[\frac{e^{-a-bp_1}}{1+e^{-a-bp_1}}\right] + I_2 \ln \left[\frac{e^{-a-bp_2}}{1+e^{-a-bp_2}} - \frac{e^{-a-bp_1}}{1+e^{-a-bp_1}}\right] + I_3 \ln \left[\frac{1}{1+e^{-a-bp_2}}\right]. \quad \text{(A.2)}$$

As before, maximum likelihood estimates are generated by selecting values of a and b that maximize this likelihood function. Again, this functional form extends easily to the general case in which k prices are offered to a sample. In the text, I refer to the second

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maximum likelihood estimator as "constrained logit," so as to distinguish it from the kinds of logit routines that presuppose equation A.1. Note that if the unconstrained likelihood function is used to estimate the price elasticity when constrained logit function is the appropriate likelihood for the data, the estimates are consistent but are inefficient for $k > 2$.

When the individual w are obtained from open-ended questions, one could maximize a likelihood function based on the probability density function for the log-logistic distribution. However, data from such survey measures typically contain zeros and probability spikes around "round" numbers (e.g., $5 or $10). For estimation purposes, continuous data were aggregated into intervals and estimated in a manner consistent with equation A.2.

APPENDIX B: NOTES ON QUESTION WORDING

A *Nylon Stockings 1963*. See Gabor 1985. N = 722.

a *Refrigerators (early 1950s)*. See Adam 1958. N = 2143. Prices listed in figure are in thousands of francs.

B *Toilet Soap 1963*. See Gabor 1985. N = 1000 (approximately).

b *New Communication Product 1988*. The exact nature of the product cannot be disclosed. Proprietary survey data N = 511. Respondents were asked how likely they were to make a purchase of this product at $70. Those who did not respond that they would "definitely" buy were offered a price of $40. These data were combined with results from another question, which asked about R's willingness to purchase this good on a monthly basis. Prices here were assigned on a random basis ($3, $5, and $12).

C *Daily Newspaper 1973*. "Would you continue to get a DAILY newspaper if the price went to 15 cents? Would you continue to get a DAILY newspaper if the price went to 20 cents?" American Institute of Public Opinion 4–7 May 1973 (AIPO 870-k). National adult sample, analysis restricted to n = 907 receiving a daily newspaper; personal interview.

c *Hardback Books 1983*. "Suppose there was a book you wanted to have which was *not* going to be coming out in paperback. If the price was $15.95/$17.95/$19.95 how likely is it that you would buy the book—very likely, somewhat likely, not too likely, not at all likely?" Gallup 29 April–7 May 1983 (See Gallup Organization 1985). National adult sample, N = 1,471; personal interview.

D *Housing 1949*. "Which of the groups on this card come closest to the amount you would be willing to pay for a new house (apartment or duplex)?" Prices listed in the figure have been divided by 100. Roper Commercial Poll no. 34, 1949. Louisville adult sample, analysis restricted to N = 135 looking for a place to live; personal interview.

d *Lottery Tickets*. See Knetch and Sinden 1984. N = 64.

E *Health Insurance 1974*. "How much would you be willing to pay a year for health insurance if it covered all of your and your's needs?" Harris Study no. 7484, March 1974. National adult sample restricted to n = 234 who currently pay for health insurance themselves; personal interview. Prices in the figure have been divided by 10.

e *BBC Channel 1 1990*. See Ehrenberg and Mills 1990. N = 213. Gabor/Granger and maximum price formats were used for purposes of statistical analysis because the one-price surveys were separated in time by several months.

F *Baseball Pay-per-View Television 1959*. "If you could watch Dodger and Giant games on pay TV—that is, where you had to pay a certain amount to watch the game—about how many games a season do you think you would watch if they cost 25 cents each. What if they cost 50 cents. If they cost one dollar." Prices listed in the figure are in cents. Field 5902, 9–14 March 1959. California adult sample, analysis restricted to N = 342 baseball fans; personal interview.

f *Swedish Comedy Show 1971*. See Bohm 1972, experiments 1 and 5. N = 62.

— *Fishing Day* (mid-1980s). See Cameron 1988, 363–64. N = 1,033. I did not present the raw data for this analysis. The method of statistical analysis (constrained logit), however, is the same as mine.

G *Safety Caps 1982*. "How much more, if any, would you be willing to pay for an over-the-counter drug that now costs $2.00 if you could purchase it in the future in tamper-proof packaging: 20 cents more or higher, 15 cents more, 5 cents more, or no extra money?" Merit 1–4 November 1982. National adult sample, N = 1,201; telephone interview.

g *Clean Up Pollution 1974–75*. See Eastman, Randall, and Hoffer, 1974–75, Table 1. N = 526. The lowest price range, from 1 cent to one dollar, is treated as 25 cents to one dollar for calculation purposes.

H *Iraq Embargo 1990*. "President Bush's economic sanctions include a ban on Iraqi oil, which could wind up raising the cost of gasoline to $2 a gallon. How much more are you willing to pay for a gallon of gas than you paid last week in order to make the ban work? nothing, 25 cents, 50, 75, or $1 or more." Gordon S. Black Corporation 8 August 1990. National adult sample, N = 561; telephone interview.

h *Fish Hatcheries (mid-1980s)*. See Cameron and Huppert 1989. Weighted N = 318.

I *World War II Effort 1943*. "In order to help pay for the war, should the Federal Government put a national sales tax of 10/5/3 cents on

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everything you buy, in addition to the taxes now in effect?" American Institute of Public Opinion 27 January 1943 (288k/t). National adult sample, N = 1,472; personal interview.

i *Iowa Gasoline Tax To Improve Roads 1954.* "Some people say we should step up the improvement of our Iowa highways. There is enough money to continue farm-to-market road building, but not enough money to continue the stepped-up improvement of Iowa's primary and main highways. An increase of two cents tax per gallon of gasoline would provide an extra 18 million dollars, enough to carry on the stepped-up rate of improvement of Iowa's main highways. Would you favor or oppose increasing the state gasoline tax 2 cents per gallon to do this? Would you be agreeable to an increase of 1 cent instead of 2 cents per gallon?" Iowa Poll no. 136, September 1954. Iowa adult sample, N = 758; personal interview.

J *Gasoline Tax To Reduce Oil Consumption 1974.* "I'm going to read you some suggestions that people have made about possible steps we could take. For each please tell me whether you think it is something that you personally would be willing to do, or not? . . . Pay 25 cents a gallon more for gasoline, with a gas tax refund on your income tax, if it would cut back our consumption of gasoline and therefore reduce the amount of money we as a nation spend on oil. . . . Pay 10 cents a gallon more for gasoline, with a gas tax refund on your income tax, if it would cut back our consumption of gasoline and therefore reduce the amount of money we as a nation spend on oil." Harris, December 1974. National adult sample, N = 1,337; personal interview.

j *Civil Defense Alarm 1963.* "Actually, the Office of Civil Defense is now developing such a new instrument to alert the population if the United States were to be attacked by an enemy. This is how the device might look. Every American home that has electricity can receive warning through this instrument. When the device is plugged into an outlet, it can be set off by a special signal that goes through the regular community power system. When this happens, the instrument makes a loud buzzing sound that will alert people to turn on their radio for emergency information. The device would be triggered by electricity in a matter of seconds after warning is received. Everything considered, how likely is it that you would get an instrument like this for your home? How about if these instruments were for sale? How likely would you be to buy one of these instruments if the cost were $5/$10/$15/$25?" Note that the sequential price format (see Appendix A) was used for purposes of presentation in Figure 2; N = 672. For the random price format (see Appendix A), N = 1,366. National Opinion Research Center, December 1963 (NORC-SRS-330 Amalgam Survey). National adult sample, N = 1,366; personal interview.

K *Cigarette Tax To Ease Budget Crisis 1987.* "Currently California's tax on cigarettes is 10 [cents] per pack. It has been proposed that this tax be increased to 20/25/30/35 per pack. If the state needed additional money and taxes had to be raised, would you favor or oppose increasing the cigarette tax to 20/25/30/35 per pack?" Field 8702, 3–10 April 1987. California adult sample, analysis restricted to N = 246 smokers; telephone interview.

k *Drinking Water 1982.* "Consumers and taxpayers end up paying the cost of cleaning up water pollution through higher prices on products and higher taxes. I'd like to know how much you might be willing to pay to clean up water pollution. In order to make sure that your drinking water and the streams, lakes and rivers in your area are clean and safe, would you be willing to pay 100 dollars a year more in taxes and higher prices, or not? Well, about how much more per year in taxes and higher prices would you be willing to pay to make sure your drinking water and the lakes and rivers in your area are clean and safe?" Louis Harris and Associates, June–December 1982. National adult sample, N = 2266; telephone interview.

L *Commercial-free Radio 1945.* "Would it be worth it to you to pay a tax of $5 a year to get radio programs without any advertising in them? [If *yes*,] Would it be worth a tax of $10 a year? [If *yes*,] Would it be worth a tax of $25 a year?" NORC, October 1945 (no. 238). National adult sample, N = 2696; personal interview.

l *Muskoka Fishing (mid-1980s).* See Kahneman 1986. N = 167.

M *Haliburton Fishing (mid-1980s).* See Kahneman 1986. N = 164.

m *Ontario Fishing (mid-1980s).* See Kahneman 1986. N = 162.

N *Environment 1970.* "To really improve the environment, would you be willing to accept an increase of $200 per year in your family's total expenses, or not? [If *no*,] Well, would you be willing to accept an increase of $100 per year in your family's total expenses to improve the environment, or not?" Note that since in contrast to k and N, the payment vehicle is ambiguous, I code this as a semipublic good. Harris Study no. 1990, January 1970. National adult sample, n = 1,686; personal interview.

n *Clean Water 1984 (Low Prices).* "Earlier I asked you about Proposition 25, the clean water bond law, which provides funds for water pollution control, water conservation, and water reclamation projects. I would now like you to think about the costs of the program to the state and to you as a taxpayer, should it pass. What if Prop. 25 would cost your household an

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additional four dollars each year for the next twenty years. If this were the cost of the program to you, would you vote for or against it? What if Prop. 25 would cost your household an additional $1/2/3 each year for the next twenty years. If this were the cost of the program to you, would you vote for or against it?" Field Poll 8406, 8–12 October 1984. California adult sample, N = 103; telephone interview.

O *Clean Water 1984 (High Prices).* "What if Prop. 25 would cost your household an additional $5/7.50/10/15/25/50 each year for the next twenty years. If this were the cost of the program to you, would you vote for or against it?" Field Poll 8406, 8–12 October 1984. California adult sample, N = 791; telephone interview.

o *Crime 1972.* "Each person in Michigan pays about $36 per year in taxes for police, courts, and prisons. How much more would you be willing to pay in state and local taxes to be used only for law enforcement and justice?" Response categories were "up to" $1, $5, $10, or nothing. It is unclear whether the prices should be treated as upper or lower boundaries. So as to err on the conservative side, we have treated these as upper boundaries: placement in the $5 category is assumed to reveal a willingness to pay of between $1 and $5, and $10 is assumed to reveal a willingness to pay of greater than $5 (since there were no higher response options). Treating these values as lower boundaries yields an elasticity of −.56. Michigan Attitudes toward Crime Study, September 1972. Michigan adult sample, N = 716; personal interview.

P *Crime 1974.* "Each person in Michigan pays about $45 per year in taxes for police, courts, and prisons. How much more would you be willing to pay in state and local taxes to be used only for law enforcement and justice? Response categories were as in o. Michigan Attitudes toward Crime Study, January 1974. Michigan adult sample, N = 797; personal interview.

p *Arts 1973.* "Would you be willing to pay an additional $50 a year in your taxes if the money were used to maintain and operate cultural facilities such as theatre, music and art exhibitions? [If *no* or not sure,] How about a tax rise of $25 to pay for such facilities, would you be willing to pay that much, or not? [If *no* or not sure,] Would you be willing to pay an additional $5 in taxes to make such activities more available?" Harris, January 1973 (see Farrell 1975). National adult sample over 16 years of age, N = 2,785; personal interview.

Q *Cultural Activities 1975.* "Would you be willing to pay $25 more in taxes per year if you knew that the money would be used to support arts and cultural activities and facilities, or would you not be willing to do that? How about a rise of $15—would you be willing to pay $15 more in taxes per year if you knew that the money would be used to support arts and cultural activities and facilities, or would you not be willing to do that? And what about a rise of $5—would you be willing to pay $5 more in taxes per year if you knew that the money would be used to support arts and cultural activities and facilities, or would you not be willing to do that?" Harris, June 1975 (A-H0027). National adult sample, N = 1,462; personal interview.

q *Arts 1984.* "The federal government now pays over $700 per capita for defense, $130 for education, and no more than 70 cents for the arts. Would you be willing to pay . . . $25/$15/$10/$5 more in taxes per year . . . for the arts, or would you not be willing to do that?" Harris 5–25 March 1984. National adult sample, N = 1,504; telephone interview.

R *Arts 1987.* "The federal government now pays over $900 per capita for defense, $140 for education, and no more than 75 cents for the arts. Would you be willing to pay . . . $25/$15/$10/$5 more in taxes per year . . . for the arts, or would you not be willing to do this?" Harris 13 March–6 April 1987 (no. 2436). National adult sample, N = 1,501; telephone interview.

r *South Carolina Environment 1971.* "Would you be willing to see an annual increase of $100 in your taxes if you could be assured that this would prevent future air and water pollution in the state, or not? [If *no*,] Would you be willing to pay $50 more in taxes to prevent air and water pollution? [If *no*,] How about $25—would you be willing to pay that much to get effective pollution control?" Harris no. 2130, August 1971. South Carolina sample, n = 803; personal interview.

S *Subway 1988.* "There has been much talk lately of building a subway in Los Angeles to reduce the traffic congestion and air pollution in that city. Suppose there were a bond issue on the statewide ballot to raise funds for a Los Angeles city subway project and that the cost to your household were an additional $5/$10/$25/$50 per year. If you were voting today, would you vote for or against such a proposal?" Field 8801, February 1988. California adult sample, N = 976; telephone interview.

s *Trees (mid-1980s).* See Brookshire and Coursey 1987. N = 48.

T *Environmental Cleanup 1990.* "Would you be willing to pay $500 more in taxes to clean up the environment if you knew the money was going to be used for environmental cleanup purposes only? Would you be willing to pay $200 more to clean up the environment if you knew the money was going to be used for environmental cleanup purposes only?" Yankelovich, Clancy, Shulman Poll, 27 No-

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vember 1990. National Adult sample, N = 1,000; telephone interview.

t *Deficit Reduction 1990.* "In order to reduce the federal budget deficit, would you be willing or not willing to pay $100 more in taxes? Would you be willing or not willing to pay $500 a year more in taxes?" CBS News 7 October 1990. National adult sample, N = 775; telephone interview.

U *Endangered Species 1989.* "How much would you be willing to have your taxes raised each year in order to save endangered wildlife like the condor or the Northern Spotted Owl—or wouldn't you be willing to have your taxes raised at all for that purpose?" Los Angeles Times 17–21 November 1989. National adult sample, weighted N = 2,155; telephone interview.

u *Homelessness 1988.* "California has many homeless and destitute people. Next November there may be a ballot initiative aimed at providing food and shelter for homeless people. What if this proposition were to cost your household an additional $5/$10/$25/$50 in taxes each year? If you were voting, would you vote for or against this initiative?" Field 8801, February 1988. California adult sample, N = 985; telephone interview.

V *Homelessness 1989.* "California has many homeless and destitute people. Suppose there were a ballot initiative aimed at providing food and shelter for homeless people. What if this proposition were to cost your household an additional $10/$50 in taxes each year? If you were voting, would you vote for or against this initiative?" Field 8903, July 1989. California adult sample, N = 853; telephone interview.

v *Clean Air 1965.* "If it would cost each family an extra $100 a year in taxes to have air pollution greatly reduced, would you be willing to accept this expense? How much would you be willing to pay?" Opinion Research Corporation 4 May–1 June 1965. National adult sample, N = 1,064 (half sample); personal interview.

W *Clean Water 1965.* "If it would cost each family an extra $100 a year in taxes to have water pollution greatly reduced, would you be willing to accept this expense? How much would you be willing to pay?" Opinion Research Corporation 4 May–1 June 1965. National adult sample, N = 1,064 (half sample); personal interview.

Five survey questions not included in the analysis are a 1976 General Electric Survey (Study no. 76-04) question about willingness to pay for electric cars, a 1989 Gordon Black survey on willingness to pay more to fight illegal drugs, a pair of 1990 *Los Angeles Times* (study no. 212) questions about willingness to pay for national health insurance and retirement home care, and a 1989 *Los Angeles Times* San Fernando Valley Survey (study no. 189) question concerning charitable contributions to build an "arts park." In each case, the use of filter questions made the data problematic.

When available, weighted survey data were used. Responses of *don't know* or *not sure* were excluded from computation when questions asked respondents to name their highest price. Otherwise, *don't knows* were treated as *not willing* unless ambiguity existed about whether people whose recorded response was *don't know* or *not sure* were in fact asked the willingness-to-pay question. In some cases, *don't know* responses were lumped together with other missing data codes.

The data used in this study, as well as the GAUSS-386 programs used to derive maximum likelihood estimates, are available on request.

Notes

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1. Technical definitions of *willingness to pay* may be found in Brookshire and Coursey 1987 and Mitchell and Carson 1989. These authors use the terms *willingness to pay* and *maximum willingness to pay* interchangeably.

2. Asking price is by no means the sole determinant of willingness to pay. Varying budget constraints and access to substitutes may produce variation in willingness to pay. In addition, people may have different conceptions of what a particular good should cost and who should pay. Generally speaking, the greater the variability in determinants of willingness to pay other than price, the weaker the price elasticity.

3. The logistic is just one of many distributions that might be used to describe the dispersion of willingness to pay. Experimentation with the normal and Weibull distributions, however, produced such similar results that I leave aside the question of which distribution best describes the data. The only other study that employs a comparable statistical approach found the logistic to have somewhat greater predictive accuracy than the normal (Cameron 1988).

4. This procedure assumes that people who are willing to pay $10 are also willing to pay $5. This assumption seems reasonable, although one must be careful to choose plausible prices. Adam (1958) and Gabor (1985) have shown that when the asking price is too low, people become suspicious about the quality of the product.

5. Consider, for example, the case in which people are asked whether they would pay one or two dollars for a gold brick. Presumably, 100% would say *yes* to both, yielding no information about price elasticity.

6. Sampling variability, of course, may account for an observed *B* of zero, especially if the range of prices is narrow. It would not be surprising, for example, to derive a *B* of zero from a small sample if the first price were $10 and the second $9.95.

7. This kind of closed-form solution is possible only in the special case in which two prices are presented to respondents. In the general case, estimates are obtained by iterative maximization of a likelihood function. Interested readers should consult Appendix B.

8. Certain alternative sources of data are less informative about willingness to pay than one might suppose. Experiments or quasi-experiments that track price changes and sales

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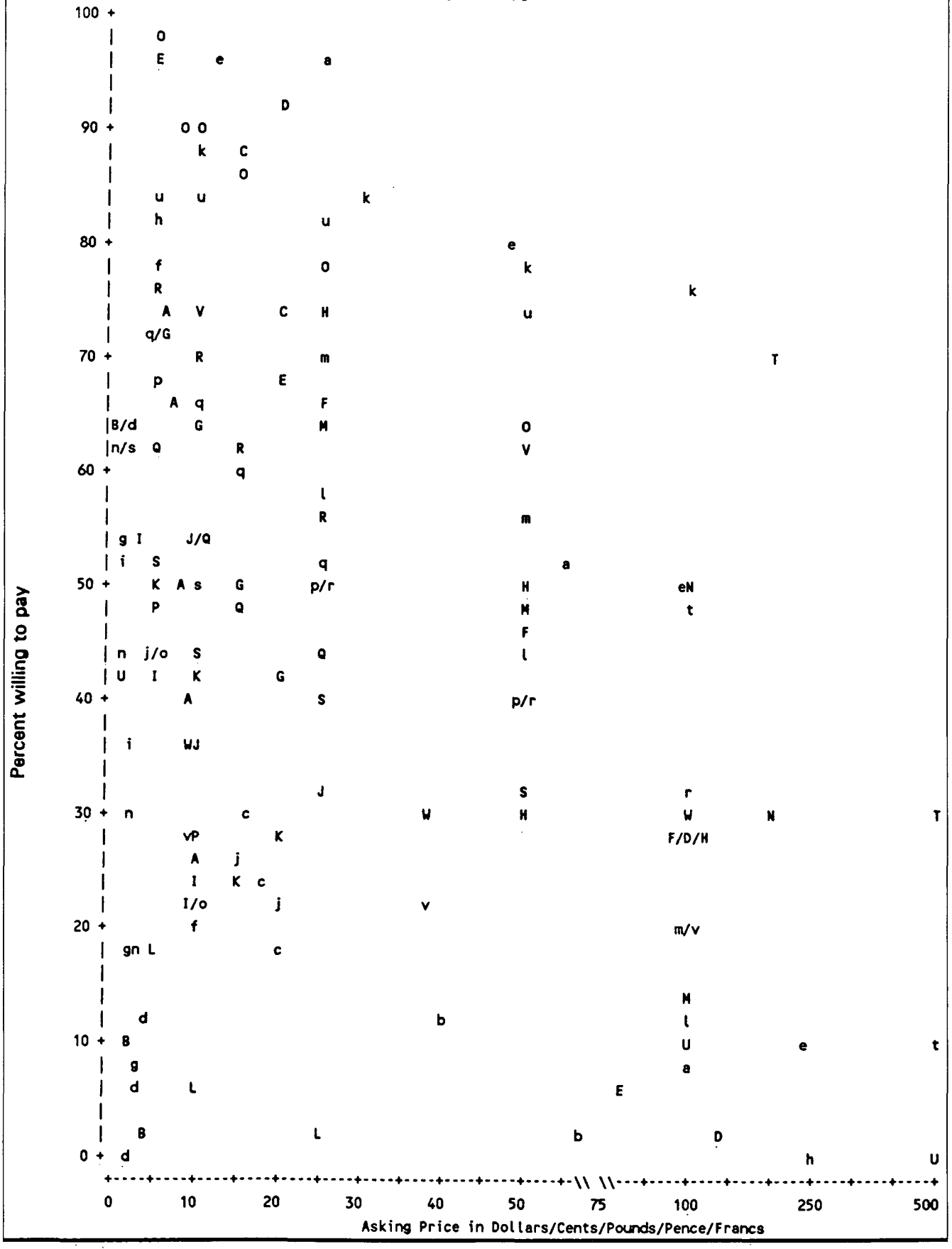
Figure B-1.

Willingness To Pay by Asking Price, by Each Survey and Type of Good



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of a given product allow one to estimate the price elasticity of aggregated demand, but not the price elasticity of willingness to pay. There are two reasons for this. First, the number of units sold does not necessarily correspond to the number of people who have made a purchase; some people may buy several units at one time (Cramer 1971). Second, those who purchase nothing go uncounted. Thus, one has no way of knowing how the proportion who are willing to pay changes with fluctuations in price. Data on local tax referenda are also unsuitable. While it might at first seem feasible to compare the level of voter support for different local tax increases, this research design has an important defect: the size of the proposed tax increase is likely to be endogenous, since those who desire a tax hike may tailor its size to the level of anticipated support. Further complications arise from local differences in turnout rates, income, and objectives of the tax proposal. The present research sheds light on the dynamics of public support for such tax increases, but voting data themselves are problematic.

9. As shown in Appendix B, a few of the questions I use ask respondents how *likely* they would be to make a purchase at a stated price. The proportion responding *definitely* or *very likely* was used as the proportion willing to pay.

10. The first is a 1988 Gallup poll that asked a series of 13 willingness-to-pay questions (see n. 24). The second is my finding that the elasticity of willingness to pay for bilingual education among Californians was a scant −.12 (Green 1988). The prices offered, however, differed from the prices offered earlier in the survey regarding homelessness and the Los Angeles subway. Respondents who had previously heard prices of $50 responded significantly more favorably toward a subsequent price of $25 than those who had previously heard $10. I dropped this case to avoid biasing the results in favor of my hypothesis concerning public vs. private goods.

11. Of the General Electric polls conducted between 1966 and 1981, 16 contain questions about willingness to pay for various public goods. However, the questions were asked only of respondents who said they would like to "see something done" in their region about each of the public goods. Thus, these data are unsuitable due to truncation bias. Elasticities calculated on these data, incidently, display the same pattern of over-time stability one observes elsewhere.

12. These results are consistent with data presented by Ehrenberg and Mills (1990, 27), which suggest that a series of prices tends to elicit more price-sensitive willingness to pay. Calculations based on their data concerning subscription television suggest that responses to a single price are about 80% as elastic as responses to a series of prices.

13. The elasticities for the two intermediate groups, "moderately frequent cultural attenders" and "infrequent cultural attenders" are −.52 and −.58, respectively.

14. For classification purposes, semiexcludable goods (i.e., goods owned collectively but consumed on a restricted basis, e.g., national parks that charge an admission fee) will be treated as public goods.

15. Complicating matters somewhat are taxes that take the form of surcharges on private goods. Consider the question of whether to raise gasoline taxes in order to reduce U.S. dependence on foreign oil. Here the question of willingness to pay involves both a public objective (limiting dependence), as well as private consumption (the gasoline one buys for personal use). Respondents may interpret this question as either one concerning willingness to pay more for gasoline or one involving willingness to pay more for energy independence.

16. In principle, these four dichotomous classification criteria generate a 16-cell typology. But as Appendix B suggests, the cases tend to cluster together, which rules out a more fine-grained examination of the role of each of the public/private criteria taken separately. I am currently collecting experimental data in an effort to understand better the distinctiveness of public goods.

17. Even more price-elastic than the private goods I included in the analysis are foodstuffs that have comparable substitutes. A February 1971 Social Science Research Council survey (Gabor 1985) found willingness to pay for butter and beans to have elasticities of approximately −10 and −5, respectively. Similarly large elasticities are to be found among Adam's (1958) consumption goods with close substitutes, such as cigarette lighters.

18. Following Cameron and Huppert (1989), I experimented with a Box–Cox transformation of price, but opted for the quadratic specification due to its superior fit to the data. In the quadratic model, the elasticity evaluated at any particular price is equal to the coefficient for ln (price) plus two times the coefficient for ln $(\text{price})^2$ times ln (price). For example, Table 2 indicates that the elasticity of willingness to pay to preserve wildlife is −.40 when the price is $10.

19. The slopes depicted in Figure 3 seem to grow weaker as price increases; but in terms of log-odds, they are in fact increasing. Note that once one includes a quadratic term it becomes necessary to decide at what price the median slope applies. For the purposes of illustration in Figure 3, this price is $10.

20. Indeed, Figure 2 gives little indication that goods offered in a fixed bundle are significantly more price elastic. Among purely private goods, the cases where quality cannot plausibly vary with price are baseball television, lottery tickets, newspapers, and a communication product; two of these four are the least price-elastic goods in this class. In the middle of the private–public continuum, only one good is offered in the form of a fixed bundle, civil defense alarms, an item that falls in the midrange of elasticities in its class.

21. Requests for data were mailed to eight firms. Three firms, each employing over two hundred people, responded. Firms B and C distribute the standard United Way solicitation cards that ask employees to make either a one-time contribution or choose one of several levels of payroll deduction (employees could also volunteer their own level of payroll contribution). Firm A distributes its own cards: hourly and weekly employees receive cards similar to the standard United Way forms; management and officers receive cards that request larger donations. The data I analyze are actual contributions (as opposed to pledges) made through the firm, whether through cash, check, or payroll deduction.

22. See n. 19. Had I omitted the quadratic term and examined only those prices at or below $50 (relegating all prices over $50 to the *$50 and over* category), the price elasticity of United Way contributions across the three firms would have been −.45 (SE = .02).

23. Both factors—the transparency of self-interest and the saliency of considerations of personal benefits—seem to account for the fact that tax initiatives, smoking restrictions, and rent controls elicit an unusually self-interested public response.

24. Normative considerations may be particularly influential when people are primed to think about the desirability of a good; hence, the unusually weak price elasticities that turn up when respondents are asked their willingness to pay immediately after stating their eagerness to see government spend more. A Gallup survey conducted in 1988, for example, asked respondents whether they would like to see more federal spending on public education. Those who answered *yes* were assigned at random to one of two tax prices: one hundred dollars or two hundred dollars. Despite the rather large sums involved, 59% of the former group and 54% of the latter were willing to pay, for an elasticity of just −.29.

25. Survey data on willingness to pay to reduce the deficit, for example, suggests that the median citizen is willing to pay $94 more in federal taxes. Since the distribution of willingness to pay is skewed, however, average willingness to pay is approximately $1,000, even assuming a rate of acceleration of −.2.

26. Willingness-to-pay surveys have a respectable track record at forecasting referendum results (Mitchell and Carson 1989). But the ballot measures that have been studied (e.g., California's Proposition 25) have been environmental bond issues that faced no organized opposition.

27. Analogous arguments have been made concerning the efficacy of cost reduction in the face of public opposition in cases where the costs are nonmonetary. McConahay (1982), for example, has shown that white opposition to court-ordered school desegregation has little to do with the degree

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of inconvenience associated with daily bus rides; he contends that tinkering with bus schedules and the location of bus stops is an ineffective means of currying public favor.

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MEASURING POLITICAL LEGITIMACY

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Political legitimacy is a key concept in both macro and micro theories. Pioneers in survey-based research on alienation and system support envisioned addressing macro questions about legitimacy with the sophisticated empiricism of individual-level methodology but failed; and a succession of innovations in item wording and questionnaire construction only led to an excessive concern with measurement issues at the individual level. I return to an enumeration of the informational requirements for assessing legitimacy in hopes of finding a conceptualization that better utilizes available survey indicators to tap relevant macro dimensions. I specify formal measurement models for both conventional and revised conceptualizations of legitimacy orientations and compare the fit of the two models systematically on data from the U.S. electorate. The revised model appears preferable on both theoretical and empirical grounds.

As one of the central concepts of political science, legitimacy would deserve sustained attention even in the absence of popular risings in Eastern Europe, China, South Africa, and elsewhere. But these events underscore the difficulty of the theoretical and methodological challenges with which legitimacy confronts scholars, and they justify renewed attention to both the potential of survey-based empirical research and the shortfalls of the conventional conceptualization.

Developing more valid measures of legitimacy orientations is not only worthwhile methodologically, it is also needed to improve our ability to distinguish among alternative explanations for important political events. The quandary of would-be reformers in the wake of Eastern Europe's democratic revolutions, whose prospects for improving their polities depend on properly diagnosing and addressing public grievances, illustrates this. At least three general interpretations of events have been offered (see Garton Ash 1990): one highlighting governmental inefficiency and incompetence, especially at economic management, one focusing on ossified and oligarchic Communist parties that stifled policy competition and upward mobility for non-party activists, and one emphasizing structural defects like the exaggerated accretion of central power at the price of regional autonomy.

Western behavioral scientists, whose national studies constitute the laboratories for new survey research, can distinguish the personal and political traits of citizens with greater or less faith in the regime and can usually situate citizen complaints along the continuum from politicians to policies to institutions (Weatherford 1987); but we lack the analytical resources to address systematically the more probing questions about institutional procedures and performance that reformers *must* address. If citizens complain about the government, for instance, are the problems with representational linkages (access and responsiveness) or with the elite policymaking process (procedural regularity and distributive fairness)? Aspects as fundamental as these are not ignored by the conventional approach to the public's legitimacy orientations, but their measurement is scattered and unsystematic. Whether this is because significant institutional reform has seldom been on the agenda or because survey researchers have focused too narrowly on measuring individuals' political traits, the upshot is that little empirical investigation has been devoted to assessing whether and how much such distinctions among aspects of institutional performance undergird expressions of political trust or alienation. I propose a revised conceptualization of the dimensions of legitimacy orientations that centers more directly on assessing these aspects of the public's image of government.

Political legitimacy is too unwieldy and complex a concept to be grappled in a frontal assault, and virtually all the empirical literature follows the tactic of breaking it into component parts. Thus, various lines of research (on alienation, political trust, modes of participation, political efficacy) all partake of a common interest in how citizens evaluate governmental authority. Many tasks can be more effectively completed by dividing the labor in this way, and time and habit invite us to take the current division as given. When we step back from these different literatures to view the global concept they all address, however, we can get a clearer perspective on the advantages and disadvantages of the conventional division.

To summarize briefly, the argument begins by distinguishing two different vantage points in the study of political legitimacy, a macro perspective emphasizing formal system properties, and a micro view emphasizing citizens' attitudes and actions. Although the early practitioners of survey-based studies of system support were motivated by the macro questions of historians and political theorists, as the methodology of survey research developed, the questions asked about legitimacy-like constructs have come to focus excessively on issues of methodology and measurement. This has weakened the ability of empirical researchers to ground individual-level findings in theories about the polity. I propose

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an operational means of integrating traditional macro concerns more systematically with the methodology and agenda of behavioral research by conceptualizing the components of legitimacy as part of a multidimensional construct, specifying this dimensional structure as a formal measurement model, and testing its empirical fit in a confirmatory covariance structure analysis of data on the U.S. public's orientations toward the political system. Throughout, the intention is to build on, rather than supplant, the thirty-year tradition of survey-based research on legitimacy orientations.

TWO PERSPECTIVES ON POLITICAL LEGITIMACY

The concept of legitimacy is problematical because it plays such a protean role in accounting for political phenomena, taking on different aspects as it links global attributes of the political system with the orientations of individual citizens. Social scientists deal with many multilevel concepts, of course; but some, like *legitimacy* and *representation*, pose more difficult analytical problems than others. Because these systemic properties have no direct individual-level counterparts, they cannot be readily conceptualized by analogy with social choice phenomena, where the complexities center on choosing the appropriate aggregation rule, given the assumption of methodological individualism. In the case of legitimacy, the proponents of both system-level and individual-level perspectives can reasonably claim pride of place for their view, although they have quite different methodological approaches, take different empirical data as evidence about legitimacy, and orient research toward different sets of theoretical questions.

The View from Above: System-level Aspects of Legitimacy

Historians and political theorists distinguish legitimate from illegitimate governments by focusing on constitutional provisions that establish the opportunity for wide public participation and ensure procedural regularity, especially provisions dealing with majority rule, minority rights, and accountability in regular and frequent elections (Dahl 1956). These features of institutional procedure provide the structures of accountability and establish incentives for the ongoing responsiveness of officials to constituents. Other theorists add criteria against which to judge the content of policy outputs, typically evaluating proposals for altering the distribution of social and economic advantages either against some standard of equality, like Rawls (1971; see Gutman 1980) or against efficiency criteria, like Burns (1963) and Sundquist (1988). The distinction between procedural and distributive justice parallels this one.

These criteria judge government performance by how well institutions and officials discern—or define—and act on the polity's best interests (Pitkin 1967, chap. 7). The literature encompasses important distinctions; but for our purposes, the shared vantage point is more important: this line of research takes for granted the epistemic assumption that an outside observer, relying on fairly gross aggregate evidence, can measure the legitimacy of a political system and rank it in comparison with other systems. Of course, detailed public opinion data are not available in most of the cases historians and political theorists need to address; but it is not just a case of sour grapes that leads to their emphasis on the privileged status of the outside observer. Researchers in this tradition do not ignore public opinion, but they do argue more or less explicitly that the beliefs of citizens at any given time are not essential information for determining the system's legitimacy (see Rogowski 1974, 4–24).[1]

The strength of the traditional approach is its broad theoretical view and its formulation of legitimacy in terms of systemic properties that facilitate comparative analysis. Four attributes stand out in this formulation:

1. *Accountability.* Are rulers accountable to the governed via a process that allows wide, effective participation?
2. *Efficiency.* Is the government set up to accomplish society's ends without undue waste of time or resources?
3. *Procedural fairness.* Is the system structured to ensure that issues are resolved in a regular, predictable way and that access to decisional arenas is open and equal?
4. *Distributive fairness.* Are the advantages and costs allocated by the system distributed equally or else deviations from prima facie equality explicitly justified on grounds that define "fair shares" in terms of some long-run, overarching equality principle?

The weakness of the traditional approach is its tendency to concentrate on formal structures and aggregate processes, and its inadequate recognition of the complementary need to observe the political system's "subjective" aspects. The signal potential of survey methodology is its promise to illuminate the nexus between institutional context and individual psychology.

The View from the Grass Roots: The Relevance of Public Opinion

The development of survey research methodology following World War II provided the tools necessary to pursue this agenda. For many of the pioneers in the development of surveys, the challenge to explain political stability offered an unparalleled opportunity for exploring the capabilities of the new method to illuminate politically significant multilevel concepts such as legitimacy (Verba 1980). Early researchers took their lead from traditional theorists like Tocqueville, whose researches shifted the ground of

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debate over political stability from formal constitutional properties to the orientations of individuals. To the allegation, widespread in his time, that democracy would lead to revolution, Tocqueville (1960) insisted instead that the careful observation of public opinion would show that far from being naturally revolutionary, the greater danger democracies face is that their citizens may voluntary withdraw from political life (see also Almond 1980). Tocqueville began his argument from the key distinction between personal and political antecedents of citizens' orientations toward government, and this division remains stalwart in the modern empirical literature.[2]

This approach highlights three aspects of the dynamics of public opinion, in explaining citizens' attitudes and actions vis-à-vis the regime. First, there are factors we would group together as part of the individual's psychological makeup, including the nonpolitical involvements that divert time and attention that might otherwise be spent on politics, along with the personal values underlying the individual's subjective sense of the importance of political participation. Hirschmann (1977), for instance, follows Tocqueville in stressing the effect of commercial and industrial pursuits in mitigating the emotion and diminishing the attention citizens give to political affairs (see Tocqueville 1960, 268–70), while Almond and Verba and later authors emphasize the role of social status and personal values (Almond and Verba, 1963, chaps. 6–9; Hyman 1966; Kluegel and Smith 1986; Mirowsky and Ross 1990). Second, there are interpersonal or social precursors of political involvement, especially the feelings of trust or suspicion that color expectations about other people and the sense of personal competence in dealing with abstract social and political forces. For instance, Noelle-Neumann (1984), like Tocqueville, focuses on the social dynamics that underlie tyranny of the majority (see Tocqueville 1960, 275–76), while others emphasize how social relations can build feelings of political competence (Almond and Verba 1963, chaps. 10, 11; Verba and Nie 1972, chaps. 9–13). Third, there is the public's sense of the capability and limits of the political process. Almond and Verba, for instance, like Tocqueville, envision a developmental process through which citizens' socialization and participation in politics nurture standards by which they judge the conduct of politicians and institutions (Almond and Verba 1963, chaps. 12–14; Tocqueville 1960, 270–77), while others envision a process more akin to consumer satisfaction, with evaluations updated and acted upon more frequently (Hirschmann 1970; Lyons and Lowery 1989; Orbell and Uno 1972).

Just as the macro perspective guides research toward the *context* within which political life takes place, the micro perspective emphasizes the reciprocal paths along which institutions shape public *orientations* and *expectations*, and public opinion then reverberates back to influence the operation of institutional processes. The prominent categories of individual beliefs are

1. *political interest and involvement*—the psychological feeling that political participation is worth the opportunity cost of trading off time and commitment from other occupations
2. *beliefs about interpersonal and social relations relevant to collective action*—expectations about the intentions and trustworthiness of other people
3. *optimism about the responsiveness of the political system*, based on citizens' perceptions of procedural and distributive fairness

It is by now a commonplace to note that the origins of survey research methodology, in sociology and social psychology, inevitably shaped the way theoretical questions were adapted to the new instrumentation. Formative initial research generally followed the sociological tradition in articulating the question of system stability in terms of political alienation, and the originators of key survey items were typically concerned with measuring the social psychological aspects of individual participation.[3] It is logical that the focus on developing measures of individual attitudes and opinions initially took precedence over theory building directed at modeling the linkage between psychological orientations and the political system; but over time, the balance between measurement and theory, at least from the perspective of the system-level aspects of legitimacy, has never been adequately restored.[4]

The Imbalance between Measurement and Theory

There is, of course, no universally appropriate equilibrium between theory building and measurement development. Presumably, any concerted research enterprise passes through multiple stages in which the relative balance shifts in one direction or the other. Observers will differ in their assessments; but most would agree that a research area is excessively measurement-driven if the bulk of activity consists of debate or experimentation involving alternative indicators and the separate measurement innovations rival one another, rather than successively contributing to the ability to explain empirical findings by grounding them in a larger theoretical context. In research on legitimacy, symptoms can be observed in the literatures on political trust, the public's perception of government responsiveness, and (to a lesser extent) efficacy and other aspects of alienation and system support. Typically, they err on the side of deferring excessively to conventional indicators or long-standing measurement models; but the overly modest use of theory even when doubts arise about traditional indicators also signals measurement-driven research.

Repeated use has inevitably had the effect of institutionalizing the conventional measures (Miller, Miller, and Schneider 1980); and some scholars have explicitly opted to take the indicators themselves (rather than their originating theoretical concepts) as the key entities for model building. Wright, for instance, suggests that available indicators should be

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considered valid because they are "commonly employed" and there is no need to raise the issue "how best to measure political alienation; the manner in which it is usually measured suffices" (1976, 91, 95). Prominent examples of more moderate versions of the tendency also appear in the literature. Consider, for instance, the controversy over alternative interpretations of the conventional multiitem index of political trust—the one school holding that it indicates public favorability toward the government's policy outputs, the other reading it as an indicator of the popularity of incumbent officials.[5] This is, by any reckoning, an important controversy: the political trust index was introduced with the intention of monitoring the core elements of public support for the regime (Stokes 1962); and the two interpretations differ (on generality and temporal stability) in a way that relates logically to the concept of legitimacy as a systemic property. And yet, if we take a broader perspective on legitimacy and its place in theories of politics, this debate appears to miss the main issues, in two ways: it begins with the measures and tries to fit theoretical inferences to them, rather than the reverse, and it promotes the question of policies-versus-incumbents by construing legitimacy in terms of public approval for governmental outputs, rather than the more theoretically central issues of how citizens evaluate the system's procedural efficiency or distributive fairness.

A more subtle example of the inclination to defer excessively to the conventional image of how theory and measurement should relate appears in two technically sophisticated methodological reviews of research on legitimacy and alienation (Mason, House, and Martin 1985; Parker 1986). Both set out to test the statistical justification for the accepted grouping of questionnaire items into indices, generally by comparing the fit of hierarchically nested models; both demonstrate the weaknesses of the simple factor analysis techniques on which initial clustering was based; and both propose slight modifications of the conventional indices justifying the newly proposed scales essentially on statistical grounds. These evaluations provide useful information; but because they start from the conventional model and limit their tests to that specification, they can yield no theoretically grounded examination of current measurement strategies. The incompleteness of this approach illustrates what Wheaton calls "model inertia": given that many different sets of assumptions can be used to identify a model, achieving a satisfactory statistical fit for a particular specification leaves open the question whether other, quite different, specifications could account for the observed data. The temptation to narrow the analytical purview is especially strong when the model under investigation enjoys conventional status and a long history: "[T]he starting point assumptions used in identifying a model tend to determine the set of restrictions considered in testing a series of hierarchically defined nested models. The effect of the initial model often goes unrecognized. The problem of model inertia, whether it is due to personal commitment or the legitimation of certain specifications in previously published research, is the sense of uniqueness attached to a model that fits" (Wheaton 1988, 196).

Less frequent than instances where indicators are reified in place of the theory are examples in which researchers are appropriately skeptical of particular measures but inadequately aggressive in recurring to theory. Research progress on political legitimacy and its cognates has been stymied in the past; and, almost without exception, the immediate inclination has been to attempt to concoct new questionnaire items to measure the same theoretical entities for which the old measures did a poor job. I do not wish to minimize the genuine dilemma researchers face in a failed replication (the evidence is always interpretable as either weak measurement or poorly specified theory) only to point out the regularity with which innovation has concentrated on measurement at the individual level, rather than returning to the (macro) theory of legitimacy or to a systematic evaluation of the measurement model.[6] Something of the same tendency is visible in the history of failed efforts to disseminate promising measurement developments, where the translation of interesting local innovations to national studies appears to have been defeated typically by the appeal to the historical continuity of the conventional constructs, rather than being joined on theoretical grounds.[7]

The etiology of the imbalance observable between theory and measurement is doubtless somewhat different in each case, but the great preponderance of recent research has been characterized by a common approach. It begins not from the global concepts (legitimacy, alienation) that figure in theories of system stability or political behavior, but, rather, from their component parts as these have been defined by the accretion of conventional indicators. Continuity across the time series is a worthy goal, but it need not dictate that new research efforts must necessarily be bound by the old measurement models. I shall now review the dimensional structure of the conventional indexes and outline an alternative measurement model that specifies more explicitly both the macro and micro aspects of legitimacy.

THE DIMENSIONAL STRUCTURE OF LEGITIMACY ORIENTATIONS

I shall pursue a program of model specification and data analysis that attempts to combine the top–down perspective of system-level theories with the bottom–up perspective of empirical public opinion studies. Although drawing insights from both vantage points, the focus is at neither pole of the continuum but, rather, on the intermediate level between them, the *measurement model*—a middle-range formulation of the linkage between the concept of legitimacy as a multidimensional theoretical entity and the empirical indicators through which we observe citizens' legiti-

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macy orientations. Measurement models are especially useful in discussing global concepts where the relationships among component parts are in question, for they provide a means of formulating theoretical statements precisely and unambiguously and of operationalizing and comparing alternative hypotheses about dimensionality. I compare two formulations: a model of the dimensional structure encapsulated in the conventional indexes of legitimacy orientations and a revised model based on the same indicators. With a concept as complex and multifaceted as legitimacy, both models propose complicated structures of relations among multiple components; universal agreement cannot be expected on the details. In order to avoid a comparison that depends on prior complete consensus, the revised model is compared with several alternative specifications of the conventional one.

The key feature of the conventional model is the separation between personal and political correlates of legitimacy orientations; within this general depiction of the concept's dimensionality, two other features are characteristic. First, the components of legitimacy (efficacy, trust, political involvement, etc.) are typically treated as if they were theoretically and statistically separable. The practice of assuming orthogonal components is usually explained on statistical grounds by pointing to the results of a simple factor analysis (see Miller, Miller, and Schneider 1980); but more discriminating statistical analysis shows that this assumption is inconsistent with the observed correlations among indices (Mason, House, and Martin 1985). The assumption is also questionable on theoretical grounds, where the traditional literature envisions the concept as a global variable comprising a nexus of several related components, at least within the "personal" and "political" categories (see Almond and Verba 1963, chaps. 8–12; Easton 1975). Second, survey-based studies typically aim at designing indicators of individuals' feelings of support or rejection of the government as a whole. Outside of the distinctions between policies versus incumbents and national versus local, the conventional indices often combine references to functions and structures that democratic theorists distinguish (e.g., bureaucracy vs. legislature, elections vs. policymaking). My intention is to work toward framing indicators of citizens' evaluations of specific political functions, particularly distinguishing procedural efficiency, equal access, distributive fairness, and accountability.

The first step in constructing a measurement model is specifying the way individual items are grouped into indices. Data come from the 1976 National Election Study—one of several from the late 1970s and early 1980s that would serve our purposes adequately, since they contain all the conventional items dating from the previous two decades, as well as some additional items carried over several of the biennial surveys. The relatively large item pool ensures more secure statistical estimation of dimensionality, while the inclusion of the main items forming the time series allows us to give appropriate weight to the claims of continuity. (Appendix A shows the conventional arrangement of legitimacy items.) Since this operationalization of the component dimensions of legitimacy is a familiar one (Miller, Miller, and Schneider 1980), it can serve as a starting point for a discussion of the revised model.

As with the conventional model, the distinction between personal and political sources of legitimacy orientations is central to the revised model. Beyond this similarity, however, the two take different paths in operationalizing the concept's dimensionality. The greater difference is in formulating indices and their interrelations for the political dimensions of legitimacy, where our interest centers on the extent to which the mass public's evaluations of governmental procedures and outputs mirror the distinctions at the heart of macro approaches—accountability, efficiency, and procedural and distributive fairness. These orientations fall naturally into two categories: those having to do with accountability and responsiveness (the linkage processes that relate the government to the citizenry) and those having to do with outputs and policymaking procedures (the performance of governments, once in power). Eventually, appropriate measurement of all these traits will depend on modifying old items and developing new ones; but I concentrate here on reconfiguring extant measures, partly in order to compare the two models in as balanced a way as possible and partly to establish the continuity between the revised model and the time series of conventional indicators.

Accountability is tapped by two of the conventional indices, *government responsiveness* and *external efficacy*. Unfortunately, these indexes combine references to the electoral process with items on officials' conduct as policymakers, distinctive aspects of the governmental process that macro theories differentiate. The revised index *accountability mechanisms* is comprised of items referring explicitly to political parties and elections as linkage mechanisms, calling on respondents to evaluate them in light of their ability to provide access and to hold officials accountable. The institutional mechanisms of accountability are crucial formal structures of legitimate government; but they are effective only to the extent that governing elites acknowledge and act on the institutional incentives intended to motivate responsiveness. Three of the conventional indicators tap citizens' judgments about politicians' actual responsiveness: *political efficacy*, *external efficacy*, and *government responsiveness*. All these, however, combine this aspect of governmental performance with other features, including personal traits of the respondent and structural traits of the government and the party system. The revised index *officials' attentiveness to constituents* more clearly taps this aspect of the representational linkage. The four items measuring this dimension all refer to the activities of officeholders to maintain attentive channels of communication open to the citizenry: two (*attention* and *don't care*) refer to government and public officials

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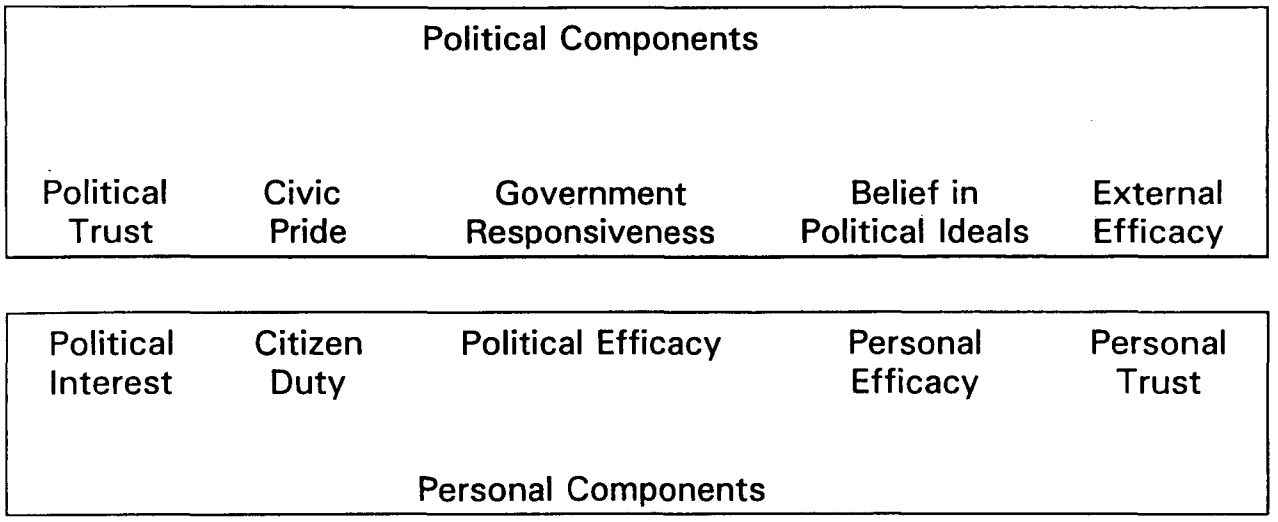
Figure 1.

The Conventional Model of the Components of Legitimacy Orientations



in general, and two (*congressmen* and *lose touch*) cite Congress members in particular.

Three additional constructs tap citizens' evaluations of the government's outputs and policymaking procedures, distinguishing between procedural efficiency and overall fairness. Procedural efficiency is tapped by three items on the conventional index *political cynicism* but there they are combined with evaluations of the government's overall performance and the fairness with which various interests are represented. The revised index *officials' competence/efficiency* groups these items (*waste, incompetent,* and *crooks*) separately to focus on individuals' judgments of the competence and honesty of officials. The public's general evaluations of governmental fairness are tapped by items included in the conventional *political cynicism* index and by the cluster *belief in political ideals*. The new index *fairness of the political process* makes a separate cluster of items that refer to the policymaking activities of the government in general, rather than the actions of individual officials. Four of these call for respondents to think broadly in terms of institutions (*trust, big interests, represented,* and *fair courts*), and these cue respondents' comparisons against various formulations of the fairness principle; the other two (*blame* and *good politicians*) refer to political leaders in general and explicitly invite respondents to distinguish in their attribution of blame between individual politicians and the government as a whole.[8] The final index among the political or systemic correlates of legitimacy orientations, *civic pride,* replicates the conventional index, clustering two items that refer to approval in a highly general and patriotic way, rather than to the activities of particular institutions or politicians.

Complementing the political components of legitimacy orientations are the personal and social correlates of citizens' evaluations of the political system. The bulk of empirical research on legitimacylike concepts over the last several decades has been concentrated in developing and improving measures of these aspects; and my revised model carries over the dimensional structure of the conventional approach with fewer changes than were needed with the political correlates. Four indices of the revised model (*political interest, civic duty, personal efficacy,* and *interpersonal trust*) are brought over from the conventional model in identical form. The remaining index, *subjective political competence,* modifies the conventional index *political efficacy* in a minor way, by pruning one item (*don't care*) referring to the conduct of officials, so that the index focuses more precisely on the interaction between the individual and the political world in general. (Appendix B illustrates the revised configuration of indicators and constructs.)

The next step toward integrating political theories of legitimacy into a complete measurement model is to specify the relationships among these constructs explicitly. The conventional model distinguishes between input and output alienation or between the personal and political sources of attitudes toward the regime. This implies that the 10 indicators can be clustered into two broad groupings. The literature is not explicit on this point; but the distinction between personal and political presumably implies that correlations within each cluster are positive, although the use of simple factor analysis results to justify de facto treatment of the 10 components as separate indexes implies that these correlations should be small. Figure 1 illustrates the conventional conceptualization of the dimensions of legitimacy orientations.

The revised model looks to the theoretical precursors of modern survey-based research; and there, one is struck by the parsimony of formulations of the key dimensions of legitimacy. The revised model clusters the 10 first-level indicators into four constructs. Sys-

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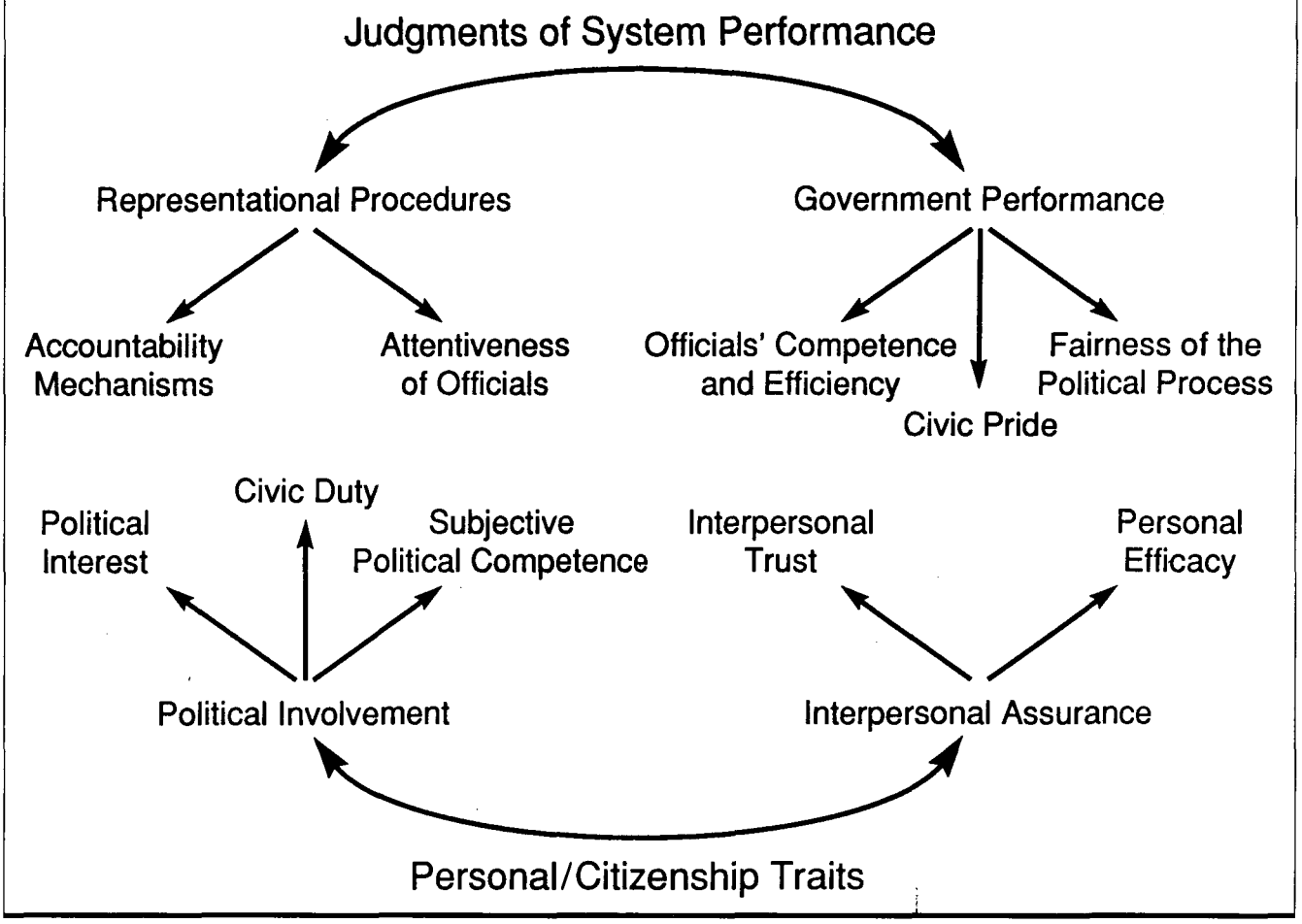
Figure 2.

The Dimensions of Legitimacy Orientations: The Views from "Above" and "Below"



tem-level theories highlight two aspects of constitutional design and institutional performance—one centered on linkage between citizens and government, the other on the conduct and outputs of the governmental process. The indexes *accountability mechanisms* and *officials' attentiveness* tap public evaluations of the quality of representational links, while *officials' competence/efficiency, fairness of the political process,* and *civic pride* tap evaluations of governmental performance. Individual-level theories similarly emphasize two fundamental springs of legitimacy orientations—one centered on personal political involvement, the other on the social sources of interpersonal assurance. The indexes *political interest, civic duty,* and *subjective political competence* tap personal participation, while *interpersonal trust* and *personal efficacy* tap the political correlates of social relations. Figure 2, an interim illustration, shows this hypothesized dimensional structure.

The figure, however, overemphasizes the separateness of macro and micro. Both perspectives (one looking to the performance of the system, the other to the character of citizens) are linked by the paths along which both citizens' feelings about themselves and their fellows generalize to confidence in the political process, and citizens' experiences with the political system condition their optimism about the prospects for successful collective action (see Almond and Verba 1963, 32–36; Barry 1970; Lijphart 1980; Pateman 1971). The links between the personal and political aspects of legitimacy and citizenship orientations are forged by a complex, dynamic process in which different streams of causation predominate at particular points in the life cycle and different causal processes proceed, each at its own characteristic pace.[9] In the cross-sectional data available, we can expect to view only a snapshot of the outcomes of those processes; but while these data cannot unravel the genesis of political support, they can illuminate the connections among its dimensions at a given time.

Four paths link citizenship orientations and evaluations of political system performance. The justification for specifying these draws on the two central themes of the civic culture literature: (1) the role of personal political efficacy and success at social relations in fostering optimism about the political system and (2) the complementary role of the perception that

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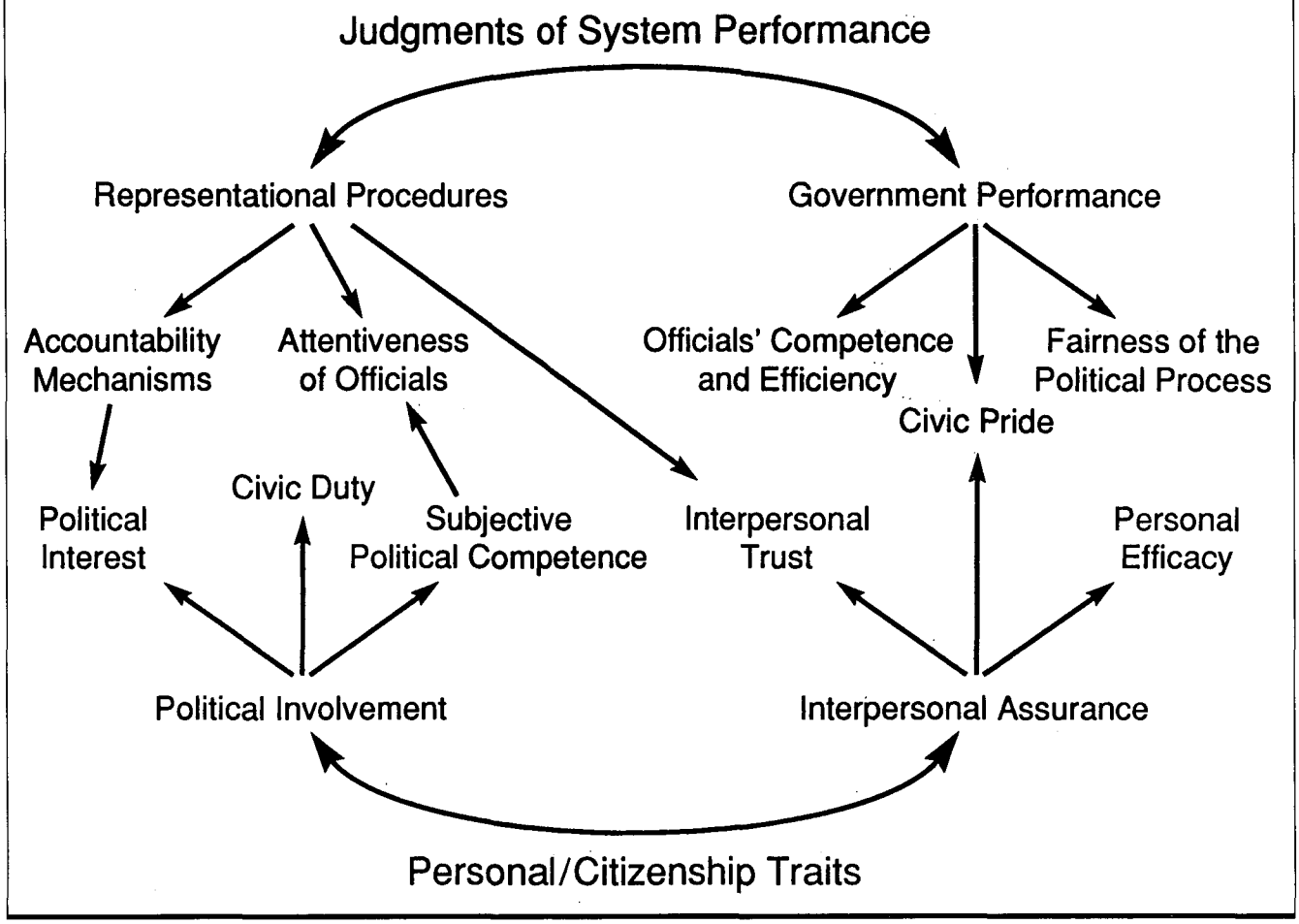
Figure 3.

The Structures of Relations Among The Dimensions of Legitimacy Orientations



the political system is open and effective in fostering personal political interest and building the foundation for collective action. Two paths specify causal flows from the personal to the political. Individuals who have a strong sense of mastery of the requisites of effective citizenship are more likely to perceive that representational mechanisms work effectively: *subjective political competence* influences evaluations of *officials' attentiveness*. And individuals who feel personally effective in their social relationships will be more likely to view the political system in a positive light: *interpersonal assurance* conditions *civic pride* (see Verba and Nie 1972, chaps. 9–13). It is logical to suppose that in the short run, the causal flow is along such routes, from relatively stable personal traits to more readily fluctuating evaluations of government; but over the long run, the polity also shapes its citizens (see Almond and Verba 1963, 493–497). Two such paths should be observable in the cross section. Citizens who perceive that the institutional mechanisms for maintaining accountability generally work effectively are more likely to take a vigorous interest in political affairs: optimism about *accountability mechanisms* fosters *political interest*. And individuals who view the government as generally representative and responsive are more likely to be optimistic about social relations and the prospects for nonpolitical collective endeavors: the perception of *governmental responsiveness* fosters politically relevant *interpersonal trust*. Figure 3 illustrates these hypotheses, specifying the second-order factor structure of the revised model.

The model summarized in Appendix B and Figure 3 has been justified and explicated by reference to previous theory and empirical literature and by noting the rationale of face validity for grouping observables with similar political content as indicators of the same theoretical construct. These are strong arguments; but on their own they cannot be definitive, for comparable claims of reasonableness and plausibility have been mobilized for the conventional specification. To be persuasive, the argument from theory and face validity must be combined with empirical evaluation. The empirical question is, If the revised conceptualization is preferable on theoretical grounds, does it comport at least as well as the conventional formulation with the way citizens think about the political system?

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TABLE 1

Fit Indices for Models of Legitimacy Orientations

	GOODNESS-OF-FIT STATISTICS*					
				INCREMENTAL FIT	OVERALL FIT	
MODEL	CHI-SQUARE	DF	CHI-SQ/DF	BOLLEN'S DELTA	BENTLER-BONNETT COMP. FIT	HOELTER'S CRITICAL N
Null Model	8467.30	630	13.44	—	—	—
*Conventional Model***						
Parallel	6933.9	648	10.70	—	.094	117.23
Tau-equivalent	3795.2	620	6.12	—	.520	205.38
Congeneric	2960.4	594	4.98	.261	.633	252.83
Two common factors	1957.92	584	3.35	.572	.821	376.18
Twenty covariances	1783.82	574	3.11	.624	.846	406.20
2 factors + 4 structural paths	1708.2	580	2.95	.649	.857	428.38
*Revised Model***						
Parallel	8071.0	648	12.46	—	.050	100.71
Tau-equivalent	3922.3	620	6.33	—	.503	198.73
Congeneric	3215.4	594	5.41	.212	.598	232.78
Full Model	1355.52	578	2.35	.768	.901	538.07
Three corr'd errors	1195.77	575	2.08	.815	.921	606.96

* See text for descriptions of fit statistics.
** Successive versions of the conventional model and the revised model specify less restrictive assumptions regarding measurement properties (parallel, tau-equivalent, congeneric) and structural properties.

EVALUATING ALTERNATIVE IMAGES OF LEGITIMACY ORIENTATIONS: THE STATISTICAL EVIDENCE

The appropriate method for comparing alternative hypotheses about dimensionality is covariance structure analysis. Ideally, it would be possible to estimate two measurement models and then to resolve the issue by comparing the magnitude of their respective fit coefficients. For both theoretical and methodological reasons, we can approach, but not achieve, this neat resolution. Theoretical differences over the specification of the two models would make it unlikely that any resolution could claim consensual agreement. Debates occur over specification among adherents to the conventional model, and readers will doubtless suggest amendments to the model proposed here. We can narrow this potential source of indefiniteness by making our assumptions explicit and by following conservative estimation practices,[10] but additional theorizing and testing will be needed before a consensual model is developed. Even with agreement on specification, however, the methodology for comparing structural equation models in situations like the present one cannot yield an unambiguous determination that one or the other should be rejected. Although several complementary goodness-of-fit coefficients are available, no one of them provides a universally appropriate criterion for choosing definitively between nonnested substantive models.[11] My analysis, therefore, utilizes several complementary statistics based on the likelihood ratio test (Bollen 1989, 289–96; Matsueda and Bielby 1986). By comparing the two specifications sequentially at several stages of the model-building process and by using multiple coefficients to assess fit, it is possible to build a sound foundation of cumulated evidence on which to judge the balance of empirical support.

The fit statistics in Table 1, which presents the data for carrying out the relevant comparisons, include indicators of both overall fit and incremental improvement in fit. As measures of overall fit, we compute chi-squared and its associated degrees of freedom, χ^2/df, the Bentler–Bonnett comparative fit index, and Hoelter's Critical N. Chi-squared remains the most commonly used fit coefficient; but its interpretation is ambiguous, since it depends not only on the fit function but also on sample size, being biased against models in larger samples (Long 1983). The ratio χ^2/df has sometimes been advocated to supplement or replace the chi-squared test, since it has an intuitive interpretation in terms of "fit per overidentifying restriction." Although various studies have suggested threshold levels of this coefficient for determining sufficient fit (Wheaton et al 1977 proposes 5; Carmines and MacIver 1981 propose 2), there is no strong rationale for the measure since it maintains chi-squared's dependence on sample size. The comparative fit index (Bentler 1990; Bentler and Bonnett 1980; Bollen 1989, 131–35, 289–92) resembles the adjusted goodness-of-fit index of Joreskog and Sorbom but has the advantage of being a slightly more conservative indicator; its usual threshold for sufficient fit is .9. Most goodness-of-fit measures are

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complicated in their construction and interpretation by the need to compensate for the confounding effect of sample size. Rather than correcting the fit function and likelihood ratio for sample size, Hoelter's (1983) Critical N index uses sample size to gauge fit. The problem is that a theoretically sound model is more likely to be rejected in a large sample; Critical N is the sample size at which the model would be accepted at a given confidence level, so that larger values of Critical N are better. Hoelter shows that a reasonable threshold for this measure is 200; I calculate Critical N at $pr(\chi^2) \geq .05$.

To these measures of overall fit, we add a coefficient of incremental fit to assess the gain from successive modification of each of the basic models. Sobel and Bohrnstedt (1984) point out that the baseline for indices of overall fit is a complete null model that is often unrealistically naive in that it takes a more theoretically agnostic position than the literature warrants. This results in a statistical criterion that produces too-ready acceptance of relatively simple models that improve on a situation of complete ignorance and too little capacity to distinguish between sequential refinements that typify a more mature research area. They propose measuring incremental improvements in fit against an "informed null" baseline (specified as a substantively meaningful but restrictive model), given current theory. My analysis employs a sophisticated factor analysis model as the baseline, and Table 1 shows incremental fit measured by Bollen's delta coefficient.[12]

Table 1 displays these fit indexes for a series of less restrictive specifications of each model. The first three are essentially factor analysis models operationalizing successively weaker assumptions about the measurement properties of the indicators (Alwin and Jackson 1979). *Parallel* measurement is the strongest possible measurement model; it assumes that the contribution of the construct to each of its associated indicators is equal and that their reliabilities are the same (i.e., either each measure of a given factor has the same loading or all the loadings equal 1.0 if the variance of the factor is a free parameter, and all the error variances are identical). *Tau-equivalent* measurement retains the assumption of equal factor loadings but allows reliabilities to differ. *Congeneric* measurement allows variation on both properties: the unobserved construct need not contribute identically to each of its measures, nor need the indicators have identical reliabilities. Following Sobel and Bohrnstedt (1984), I specify tau-equivalent measurement as the appropriate informed null baseline.

I first review the statistical evidence for each model separately, then return to their comparison. The upper portion of Table 1 shows goodness-of-fit statistics for the conventional model. The conventional approach has typically treated political trust, political efficacy, governmental responsiveness, and other indices as independent measures of discrete traits; and if this formulation is taken as definitive, then the orthogonal factor structure specified by the congeneric measures model is the appropriate operationalization. Hoelter's Critical N, which comfortably exceeds the threshold value of 200, implies that this model fits the data adequately; but none of the other indices supports this inference.[13] Although a useful starting point, this is arguably too conservative an interpretation of the model for the conventional indices: the assumption of orthogonality has never been universally accepted. At the same time, there is no strong warrant for any particular formulation of how the conventionally measured components are related and hence no consensus as to the route along which the model could be improved by freeing specific sets of restrictions. The general distinction between input and output alienation suggests two clusters of indicators (one for "personal," the other for "political," sources) and thus provides a theoretical rationale for respecifying the model (see Bollen 1989, 296–305). Following this lead, the empirical relations within each cluster could be specified as covariances or as causal paths. The causal hypothesis would be that a single construct gives rise to the five personal traits, while a separate orientation causes variation in the five indicators of political evaluations. Fit statistics for this specification appear in row 4, the conventional model, where 10 structural paths are estimated as free parameters. The correlational variant hypothesizes that the five indicators in each cluster are directly linked but does not assert the operation of a common factor. Within each cluster there are 10 pairwise covariances, and fit statistics for this less restrictive specification are given in row 5.

Table 1 shows goodness-of-fit coefficients for these two specifications. Both formulations are built around the conventional arrangement of items into indices, abandon the assumption of orthogonal indicators, and produce clear improvements in fit, as shown by Bollen's coefficient of incremental fit, which increases from .26 to .6.[14] Either seems a fair representation of the general thrust of the conventional model's conceptualization of the dimensions of legitimacy orientations. Although consensus is lacking on the way personal traits might relate to system orientations (measured by the conventional indicators), a more generous test of the conventional model would allow some structural links to be estimated as free parameters. The final row under the conventional model shows fit statistics for such a model, estimated with four free structural parameters.[15] The overall fit of each of these models would be judged acceptable on the basis of the Critical N index, but they fall short of sufficient fit when compared with the Bentler–Bonnett statistic or the more familiar χ^2/df.

The lower portion of Table 1 arrays corresponding statistics for the revised model. As with the conventional model, we see steady improvement in fit moving from more restrictive to less restrictive factor analysis specifications. Moving from these stock formulations to estimate the full version of theoretically derived model (depicted in Figure 3) results in a pronounced improvement in fit, with Bollen's coefficient of incremental fit increasing from .21 to .77. The

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coefficients of overall fit tell the same story: χ^2/df and the Bentler–Bonnett comparative fit index move up to their threshold values for sufficient fit, while Critical N comfortably exceeds its threshold value.

Because the conventional and revised models are not nested, precise comparisons of fit statistics cannot be taken as definitive. Nevertheless, comparability is strengthened by the fact that they are estimated on the same population and specified with approximately equal numbers of free parameters. Without placing too much weight on any one measure of fit, the consistency across all the coefficients is impressive in supporting the empirical superiority of the revised model.

The last row of Table 1 shows goodness-of-fit statistics for a further specification of the revised model, this one freeing covariances across three pairs of error terms. The rationale for testing an explicit model of measurement error returns us to our initial agenda. I have stressed throughout the desirability of formulating a new model of the dimensions of legitimacy orientations that builds on previous work and whose analysis can make use of the available time series of survey data. Building up the revised model from items originally designed to tap the conventional formulation serves this purpose; but it arguably also produces a very conservative test of the empirical support for the revised model, for reasons springing from both the content of the items and the sequence of questions on the survey. Specifically:

> The new indicators are operationalized with items purposely constructed to measure the old indices. Over the years, initial items have been dropped or reworded and new items added, with an eye to improving the scalability and orthogonality of the conventional indices. The extent to which these changes have been successful will register in higher inter-item correlations for the conventional indices.
>
> The data were generated by a survey designed with the conventional indices in mind; specifically, each of the conventional indices is presented to the respondent as a sequence of items, with all the items typically having similar format and wording. The adjacency and similarity of the items increase the likelihood that some of their observed correlation is due to patterned responses induced by the questionnaire instrument (see Sears and Lau 1983).

If such measurement artifacts do inflate correlations among items on the conventional indices, then reconfiguring the items into new indices may worsen the fit of the model, because it moves this artifactual correlation from within indices to between indices. The final row of Table 1 shows goodness-of-fit statistics for the model in Figure 3, now estimated with three error covariances specified as free parameters. The resulting improvement in fit (Bollen's delta increases from .77 to .82) underscores the conservative nature of our test of the revised model's empirical support.[16] Estimated on data that make the statistical test a conservative one, the revised model appears to fit the data well, with a Critical N over 600 and a comparative fit statistic of .921.

CONCLUSION: IMPLICATIONS OF THE REVISED MODEL FOR RESEARCH ON SYSTEM SUPPORT

The research agenda guided by the conventional model has produced an admirable track record of findings on the mass public's feelings of trust and favorability toward the government. We can use the conventional indicators, for instance, to discover whether citizen complaints about government center on officials or policies; and we can parse them for detailed information about the social characteristics and political self-images of supporters and skeptics. These are important questions about public opinion, and answering them should eventually help in explaining and predicting political behavior. To date, however, the conventional formulation has not delivered on its promise to connect survey-based research on legitimacy with the macro issues at the heart of theories of system stability.

The reason for this is unlikely to be inadequate inventiveness in formulating specific items on the survey questionnaire, since a host of measurement innovations have sought improvement along that route. I have pursued the alternative strategy, returning to macro theories of political legitimacy for inspiration in reconceptualizing how the multiple components of this global construct fit together and how they might best be measured. When we view the shortfall between survey-based findings and system-level inferences from the perspective of macro theories rather than survey methodology, the resulting model of the dimensions of legitimacy differs correspondingly. The distinction between the revised model and the conventional one is relatively small where the conventional approach is at its strongest—in measuring personal political orientations and mapping the way social relations and interpersonal trust condition political attitudes. The distinction is marked where the conventional model does a poorer job—in targeting the aspects of political legitimacy central to system-level theories, accountability, efficiency, and procedural and distributive fairness.

Figure 3 presents the revised formulation as a covariance structure model. Several differences distinguish it from the conventional model. Most importantly, where the conventional model implies that five separate dimensions comprise the nexus of "political" sources of legitimacy orientations, the revised model groups a modified set of five first-stage indicators into two second-stage clusters, one highlighting representational institutions and practice, the other the policy performance of officials and institutions. The conventional model's five dimensions of personal/citizenship traits are similarly grouped by the revised model into two second-stage clusters—one focusing on political interest and subjective competence, the other on trust and efficacy in social relations. Finally, the new model explicitly specifies two pairs of structural paths spanning the personal and political. Figure 4 shows maximum likelihood

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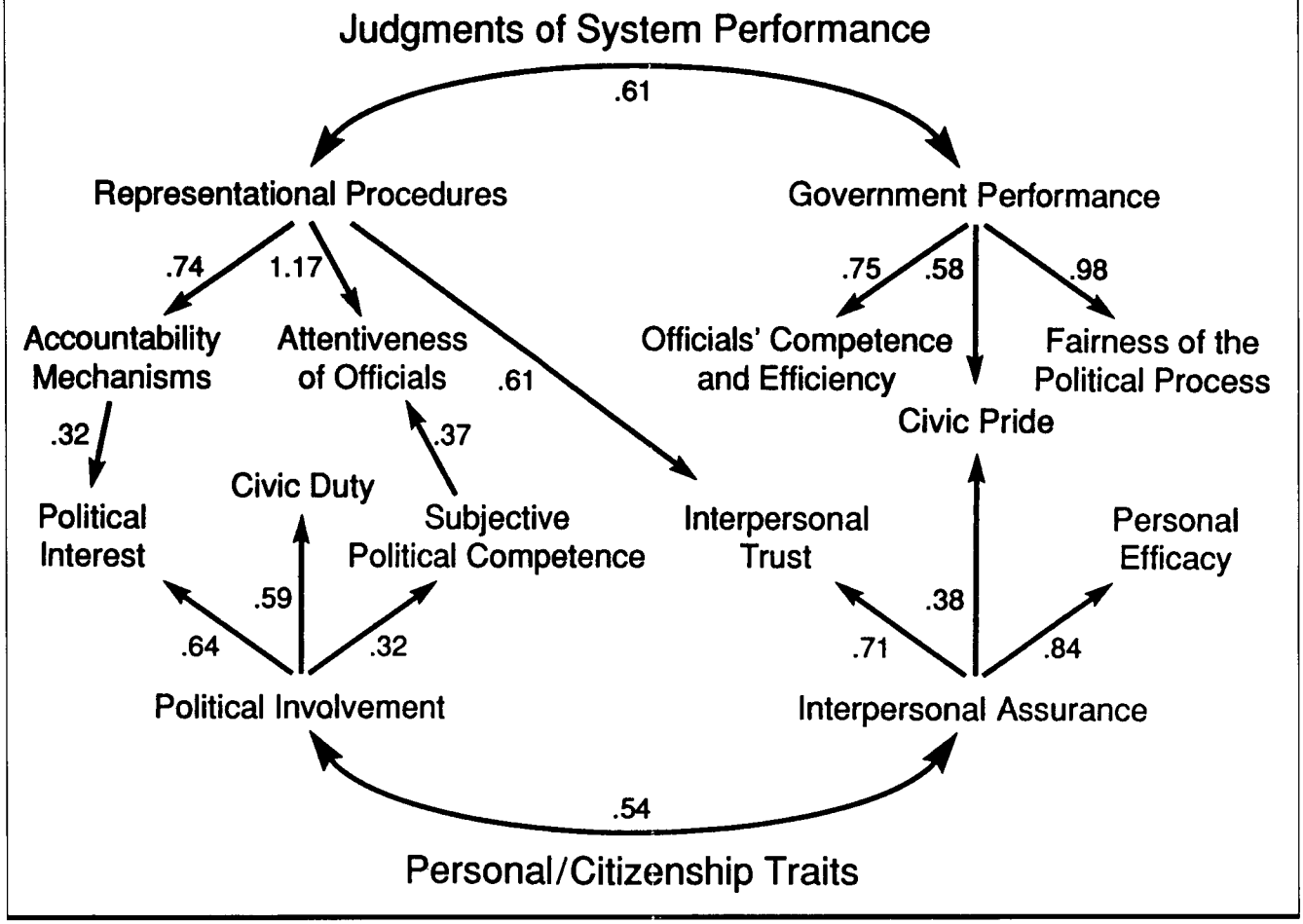
Figure 4.

The Dimensions of Legitimacy Orientations: Parameter Estimates



estimates of the structural parameters of this model, and several comments can be offered on these results.

I begin with the personal and citizenship traits at the bottom of the figure. The second-stage factor *political involvement* gives rise to a trio of indicators of the behavioral and attitudinal correlates of participation. *Political interest* and *civic duty* index active involvement or the feeling of responsibility to participate, and they dominate this factor; *subjective political competence*, which refers not to behavior but to attitudes that indirectly underlie involvement, shows a somewhat weaker but still significant causal relation to the common factor. The second-stage factor *interpersonal assurance* underlies two sets of social experiences that build (or subvert) the confidence to participate in collective political pursuits. *Interpersonal trust* taps the extent to which other people are viewed as reliable and well intentioned, while *personal efficacy* indicates the individual's sense of mastery in organizing and planning his or her life. The two second-stage factors, *political involvement* and *interpersonal assurance* are correlated as part of a general nexus of nonpolitical sources of legitimacy orientations, but the fact that they share only about a quarter of their variance (r = .54) gives empirical support to the specification of separate social and individual components.

Turning to the political sources of legitimacy orientations, the second-stage factor *representational procedures* underlies two indicators of citizens' evaluation of the effectiveness of elite–mass linkages. *Accountability mechanisms* indexes the extent to which the individual sees elections and party competition as institutions that press politicians to pay attention to their constituents, while *attentiveness of officials* taps the public's judgment about whether politicians actually do take proper account of citizens' views. The relative magnitudes of their structural parameters suggests a sensible evaluative rubric that places actions above formal intentions. That the representational connection is a key aspect of citizens' views of the system's legitimacy is underscored by three significant structural paths linking the political and personal, suggesting that (1) personal political interest is sustained in part by the impression that electoral competition heightens accountability; (2) the more politically competent the citizen feels, the more optimistically he or she will view the responsiveness of public officials; and (3) the more effectively the system's representational institutions work to con-

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nect citizens meaningfully to the world of politics, the more optimistic they are likely to be about the prospects for collective social efforts.

The second-stage factor *government performance* underlies citizens' responses to three indicators of the quality and fairness of the policy process and its outputs. *Officials' competence* taps the public's judgment about the efficiency and honesty of politicians and bureaucrats, while the more strongly normative index of *fairness of the political process* reflects citizens' sense that the policy process equitably reflects the needs of the unorganized and the organized, private interests and the public interest. *Civic pride* taps a relatively abstract sense of the political system's performance. Given the rather vague way this indicator refers to particular institutional functions, it is likely that individuals' reports only partly reflect their observations of government and are partly the product of long-standing orientations toward politics and society, a hypothesis supported by its relatively weaker connection with the *government performance* factor and by the significant path from *interpersonal assurance.*

As with the individual and social components of the "personal" sources of legitimacy orientations, the two components of their "political" antecedents, *representational procedures* and *governmental performance,* are correlated. But again, the fact that they share just over a third of their variance (r = .61) leaves a good deal of empirical support for the hypothesis that linkage institutions and policymaking performance are distinct objects of evaluation. In sum, the revised model of legitimacy orientations appears preferable to the conventional formulation on theoretical grounds, for its more complete and explicit specification of both macro and micro views of the concept's key dimensions, as well as on empirical grounds, where both the plausibility of its estimated parameters and its consistently better overall fit support its preferability.

The new model has implications both for understanding the interplay between the public and the government and for developing better measures of legitimacy and its cognates. By explicitly importing macro theories of legitimacy into the conceptualization that guides data collection at the individual level, the revised model implies a significant change in the researcher's expectations about the sophistication of ordinary citizens' evaluations of governmental performance. The conventional model directs greatest attention to individuals' reports of their personal feelings about their own participation or about their general favorability toward politicians and policy outputs. The revised model purposely constructs indicators to tap how the public's judgments go beyond such general and personalized ratings to evaluate government's procedures and performances against more impersonal normative standards. By stepping back from the systems theory image of government as a black box exchanging favorable policy outputs for the public's supportive inputs, the revised model presupposes that individuals can, and often do, go well beyond general approval or disapproval in their evaluations of political objects—that they distinguish not only between incumbents and structures but among procedures and policies, fairness, efficiency, and venality. Political theories of legitimacy rest on such subtle distinctions, but they often implicitly assume that expert outside observers are uniquely qualified to draw them. The empirical analysis presented here suggests that citizens, acting as "naive moral philosophers" (Tyler 1984), may be capable of some of the same distinctions.

Finally, several implications follow for the choice of measurement strategy and research design in studies of legitimacy and political system stability. Future research can build on this work in several directions. The model formulated here should be estimated on U.S. data collected at other times, and its applicability assessed outside the national context. But we clearly have only begun to fill out the recipe for closing the gap between theory and operationalization in survey studies of legitimacy. Researchers will need to concentrate on developing measures that tap the new indexes at least as accurately as traditional questionnaire items tapped the conventional indexes. Some promising lines of current research include studies of citizens' responses to their encounters with the bureaucracy, legislators, and the courts (see Jacobson 1987, chap. 5; Katz, Kahn, and Gutek 1975) and studies of citizens' conceptions of social and political justice (Alves and Rossi 1978; Jasso 1980; Jennings 1991; Rainwater 1974; Rasinsky 1989).

Public opinion research on political legitimacy has languished as measurement developments have proceeded independently of the macro context of theory and comparison from which legitimacy concerns gain their significance. The gap between theory and measurement will not be closed until we can estimate models containing good measures of both micro and macro variables. By proposing a new measurement model that more closely aligns survey measures with system properties, I suggest an initial step toward that goal.

APPENDIX A: NES (1976) ITEMS MEASURING LEGITIMACY ORIENTATIONS, CONVENTIONAL INDICES

Numbers in parentheses are factor loadings (lambdas) from the confirmatory covariance structure analysis of the final version of the conventional model.

Political Efficacy

No say. People like me don't have any say about what the government does. (1.0)

Voting. Voting is the only way that people like me can have any say about how the government runs things. (.727)

Complex. Sometimes politics and government seem

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so complicated that a person like me can't really understand what's going on. (.505)

Don't care. I don't think public officials care much what people like me think. (.963)

Political Cynicism or Trust

Waste. Do you think that people in government waste a lot of the money we pay in taxes, waste some of it, or don't waste very much of it? (.539)

Trust. How much of the time do you think you can trust the government in Washington to do what is right—just about all of the time, most of the time, or only some of the time? (.637)

Big interests. Would you say that the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit for all people? (1.0)

Incompetent. Do you feel that almost all the people running the government are smart people who usually know what they are doing or do you think that quite a few don't seem to know what they are doing? (.613)

Crooks. Do you think that quite a few of the people running the government are a little crooked, not very many are, or do you think hardly any of them are crooked at all? (.659)

Trust none. This scale combines responses from three items asking, Which part of the government on this list do you most often trust to do what's right . . . Congress, Supreme Court, President, political parties? Which do you trust next most often? Which do you least often trust? Scoring arrays institutions in terms of their link to "the electoral connection," with low scores indicating greatest trust in the Supreme Court, then the president, Congress, and the political parties. (.591)

Government Responsiveness

Attention. Over the years, how much attention do you feel the government pays to what people think when it decides what to do: good deal, some, or not much? (.994)

Parties. How much do you feel that political parties help to make the government pay attention to what the people think: a good deal, some, or not much? (.960)

Elections. And how much do you feel that having elections makes the government pay attention to what the people think: a good deal, some, or not much? (1.0)

Congressmen. How much attention do you think most congressmen pay to the people who elect them when they decide what to do in Congress: a good deal, some, or not much? (.942)

Personal Efficacy

Expected. When you do make plans ahead, do you usually get to carry out things the way you expect, or do things usually come up to make you change your plans? (.981)

Work out. Have you usually felt pretty sure your life would work out the way you want it to, or have there been times when you haven't been sure about it? (1.0)

Plan life. Do you think it's better to plan your life a good way ahead, or would you say that life is too much a matter of luck to plan ahead very far? (.844)

Run life. Some people feel they can run their lives pretty much the way they want to; others feel the problems of life are sometimes too big for them. Which one are you most like? (.947)

Satisfying life. In general, how satisfying do you find the way you're spending your life these days? Would you call it completely satisfying, pretty satisfying, or not very satisfying? (.494)

Personal Trust

Personal trust. Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people? (.906)

Helpful. Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves? (1.0)

Advantage. Do you think that most people would try to take advantage of you if they got the chance, or would they try to be fair? (.866)

Belief in Political Ideals

Represented. How much do you feel that you and your friends are well represented in our political system? (.981)

Fair courts. How much of the time do you think that the courts in the United States guarantee everyone a fair trial? (1.02)

Good politicians. To what extent would you say that the leading politicians in the United States have had good intentions? (1.0)

Blame for corruption. There has been a good deal of talk these days about Watergate, sex scandals, and corruption in the government in Washington. Do you think that these problems are just the result of what individual politicians have done, or is it because there's something more seriously wrong with government in general and the way it operates? (.607)

Political Interest

Follow politics. Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs most of the time, some of the time, only now and then, or hardly at all? (.575)

Campaign. Some people don't pay much attention to the political campaigns. How about you? Would you say that you have been very much interested, somewhat interested, or not much interested in following the political campaigns so far this year? (1.0)

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Care who wins. Generally speaking, would you say that you personally care a good deal which party wins the (presidential/congressional) election this fall or that you don't care very much which party wins? (.454)

Political Efficacy (External)

Lose touch. Generally speaking, those we elect to Congress in Washington lose touch with the people pretty quickly. (.805)

Not opinions. Parties are only interested in people's votes but not in their opinions. (1.0)

Citizen Duty

Can't win. It isn't so important to vote when you know your party doesn't have any chance to win. (.685)

Not important. So many other people vote in the national elections that it doesn't matter much to me whether I vote or not. (1.0)

No care. If a person doesn't care how an election comes out, he shouldn't vote in it. (.601)

System Evaluation

Proud. Would you say . . . "I am proud of many things about our form of government" or "I can't find much in our government to be proud of." (1.0)

Need change. Some people believe a change in our whole form of government is needed to solve the problems facing our country, while others feel no real change is necessary. Do you think a big change is needed in our form of government, or should it be kept pretty much as it is? (1.0)

APPENDIX B: NES (1976) ITEMS MEASURING LEGITIMACY ORIENTATIONS, NEW INDICES

Numbers in parentheses are factor loadings (lambdas) from the confirmatory covariance structure analysis of the final version of the revised model.

Political Interest

Care who wins (.416); *campaign* (1.0); *follow politics* (.857); *complex* (.570).

Citizen Duty

Can't win (.631); *not important* (1.0); *no care* (.560).

Personal Efficacy

Plan life (.839); *expected* (1.005); *work out* (1.0); *run life* (.952); *satisfying life* (.506).

Interpersonal Trust

Personal trust (.910); *helpful* (1.0); *advantage* (.872).

Civic Pride

Need change (.481); *proud* (1.0).

Subjective Political Competence

No say (1.0); *voting* (1.085); *complex* (.476).

Accountability Mechanisms/Parties and Elections

Parties (.926); *elections* (1.0); *trust none* (.371); *no say* (.788); *not opinions* (1.217).

Officials' Attentiveness to Constituents

Attention (.581); *congressmen* (.557); *don't care* (1.0); *lose touch* (.732).

Competence/Efficiency of Officials

Waste (.511); *incompetent* (.890); *crooks* (1.0).

Fairness of the Political Process

Trust (.690); *big interests* (1.0); *blame for corruption* (.657); *represented* (.727); *fair courts* (.746); *good politicians* (.726).

Notes

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1. The normative force exerted by the critique in terms of legitimacy is owed in part to the fact that the legitimacy of a regime is somehow "above" or "apart from" transient public attitudes. The evidence typically offered for the statement that public opinion is neither necessary nor sufficient for legitimacy is the fact that from time to time, regimes consensually considered legitimate in the eyes of outside observers are overthrown in an excess of revolutionary fervor, while others that observers would agree are illegitimate persist, relying on force or enculturation.

2. For instance, Almond's adaptation of Parsons and Shils's model focused attention on individual values, self-image, and social relations (Almond 1956; Almond and Verba 1963), while Easton's (1957, 1965) elaboration of the systems theory metaphor emphasized subjective responses to policies. The distinction between "input alienation" and "output alienation" (Gamson 1968, 42) is roughly coterminous with this one, and it stands as one of the relatively few strongly validated propositions in the literature (Aberbach 1969; Abravenel and Busch 1975; Converse 1972; Fraser 1970; Paige 1971; House and Mason 1975; McPherson, Welch, and Clark 1977).

3. In 1952, scales for political efficacy and citizen duty were introduced (Campbell, Gurin, and Miller 1954); and in 1958 an index of "basic evaluative orientations toward the national government" was developed (Stokes 1962, 64). In 1958 and 1964 additional items were added measuring personal efficacy

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and interpersonal trust (Rosenberg 1956) and the responsiveness of political institutions. Mason, House, and Martin (1985) review the literature on political alienation.

4. For instance, since the early 1980s, the Board of Overseers for the National Election Studies has curtailed data collection on a growing fraction of the long-standing items measuring citizens' political trust, efficacy, perception of government responsiveness, civic pride, etc. But the mood of these decisions has not been one of accomplishment at successfully completing a research agenda but, rather, frustration at the poor theoretical potential of the conventional conceptualization of legitimacy phenomena and the measurement strategy following therefrom.

5. E.g., Abramson and Aldrich 1982; Abramson and Finifter 1981; Citrin 1974; Citrin and Green 1986; Easton 1975; Erber and Lau 1990; Feldman 1983; Green 1985; Hill 1982; Miller 1974a, 1974b; Muller and Jukam 1977; Sniderman 1981; Sniderman et al. 1975; Weatherford 1984, 1987; Williams 1985.

6. From this perspective, the new items included in the 1987 pilot study constitute a variation on the same theme, for the most part presenting subtle shadings on a familiar battery of questions centered on the policies–incumbents distinction.

7. The Berkeley Social Indicators Project appears to be an example of such a local study piloting several promising measurement ideas. (Sniderman 1981 analyses a portion of the original study.) Some have been adopted, but others appear to have been rejected without much theoretical discussion.

8. It is worth noting that many of the most useful indicators of this dimension have not achieved the status of regular inclusion in NES surveys beyond the early 1980s. This dearth is not as serious as it would be if the NES were the only locus of survey innovation, since researchers in several other centers have carried forward work on related concepts (particularly public conceptions of justice—see Jennings 1991; Rasinski 1987; Tyler 1984). The measurement development work of these and other researchers can be usefully implemented to improve studies of legitimacy.

9. For instance, Almond and Verba (1963), Hyman (1959), Jennings and Niemi (1974), and Sears, Hensler, and Speer (1979) emphasize the influence of socialization and stable personal traits over a relatively long time horizon, while Hirschman (1970), Lyons and Lowery (1989) and Orbell and Uno (1972) emphasize short-run evaluations.

10. Perhaps the most common form of "specification abuse" (Wheaton 1988) is to fit correlated errors on purely statistical, rather than theoretical, grounds. Most reasonable covariance structure models can be fit very snugly to the original data if the researcher is sufficiently generous about freeing correlations between error terms. But correlated errors can mask alternative substantive specifications, hence they should be justified on theoretical grounds. I estimate each of the two models with no correlated errors, freeing covariances between errors in a separate step and explaining the rationale by reference to an explicit hypothesis about sources of measurement error.

11. Model B is *nested* in model A if B contains restrictions on parameters that are free in A but is otherwise identical. Sequential versions of the revised model and of the conventional model are nested, but the revised is not nested in the conventional, or vice versa. Key contributions to the large technical literature on comparing latent variable models include Bentler and Bonnett 1980; Hoelter 1983; Matsueda and Bielby 1986; and Sobel and Bohrnstedt 1985. Wheaton (1988) and Bentler (1990; see also Bentler and Chou 1988) provide useful brief reviews; and Bollen's text gathers several different lines of development into a unified presentation (1989, esp. chaps. 6 and 7).

12. Bollen's modification of the Bentler–Bonnett delta diminishes the coefficient's dependence on sample size and on differences in degrees of freedom between successive versions of the model (Bollen 1989, 271–2).

13. Additional support might be inferred from the ratio χ^2/df, although the value of this index (barely below its more liberal threshold at 5) and its relatively weak statistical rationale suggest that this evidence be treated circumspectly. Mason, House, and Martin (1985; cf. Parker 1986) analyse the orthogonal factors model in detail, finding it inappropriate on several counts.

14. This is consistent with the findings of Mason, House, and Martin (1985).

15. The free paths, chosen for their theoretical plausibility, are from personal and political efficacy to external efficacy, from citizen duty to civic pride, and from personal trust to political trust. Only four free structural paths are estimated in order to maintain comparability with the revised model, where the same number are estimated (since the overall fit of covariance structure models can typically be improved simply by freeing more parameters).

16. While measurement artifact appears the most likely explanation of this improvement in fit, this respecification does not test uniquely for that. Even if the context of measurement inflated inter-item correlations for the conventional indexes, this would not necessarily produce better fit in a complex model estimated by full-information methods. Nor does a significant error covariance necessarily indict measurement artifact of the sort Sears and Lau (1983) find. An error correlation indicates that two measures share variance not accounted for by the latent variables specified in the model. Measurement artifact is a prime source of such shared variance; but it could be due to any other unmeasured and unspecified construct.

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RESEARCH NOTE

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MEASURING POLITICAL VIOLENCE AND LAND INEQUALITY IN CENTRAL AMERICA

CHARLES D. BROCKETT *University of the South*

The value of cross-national quantitative studies of the relationship between mass political violence and land inequality is challenged along three lines. First, gross and systematic errors in the political violence data of the World Handbook of Political and Social Indicators *(the usual data source for empirical studies) render them worthless for Central America at least and probably much of the Third World as well. Second, conceptualizations of land inequality have been too simplistic to be of much theoretical value. Third, the temporal nature of this relationship has been inadequately considered. Responding to such deficiencies, I elaborate a broader understanding of land inequality and provide a fuller discussion of the temporal nature of its relationship to political violence. Throughout, the five nations of Central America are utilized for appropriate case material.*

Land is the most important resource in rural societies, and access to sufficient land is invariably the fundamental desire of most peasants. Accordingly, it has long been thought that the maldistribution of land is the key determinant of rural unrest, particularly unrest in its most dramatic forms, rebellion and revolution. Land maldistribution can have different manifestations. The most common aspects cited are landholding patterns so concentrated that either (1) many peasants are left with *minifundios* too small for family support (the *minifundización hypothesis*) or (2) that many peasants own no land at all (the *landlessness hypothesis*). In the most current literature the first position is argued by Midlarsky (1988) and the second by Prosterman and Riedinger (1987).

However, Muller and Seligson claim that this conventional wisdom is wrong (1987, 433–34). Their cross-national longitudinal data analysis of 85 states finds both that landlessness is irrelevant as an explanation for political violence and that land inequality (as measured by the Gini index) has only weak predictive value. Instead, they discover a stronger relationship for mass political violence with national income inequality (again measured by the Gini index).[1] They conclude, therefore, that agrarian inequality is relevant to mass political violence "only to the extent that it is associated with inequality in the nationwide distribution of income" (p. 443).

This controversy has important policy ramifications, as the various authors point out. Land redistribution (and associated support programs) is often promoted as a prophylactic against rural disorder and instability.[2] Muller and Seligson's sophisticated analysis appears compelling, thereby questioning the adequacy and wisdom of land redistribution. However, in reality, their study is flawed because of the meaningless data used to measure their dependent variable, mass political violence. The same flaw also negates Midlarsky's analysis, as in fact it does most empirical data analyses on this subject.[3] Their shared deficiency is their reliance on the grossly inaccurate data set provided by the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983). In addition, I shall also argue that previous studies are inadequate in their conceptualization of the independent variable. Land inequality cannot be adequately captured in a single dimension no matter how sophisticated the measure but must be understood instead as multidimensional in nature.[4] It is my argument, then, that the empirical relationship between land inequality and political violence remains undetermined.

MEASURING MASS POLITICAL VIOLENCE

> February [1977] An army base is set up near San Juan Cotzal [Guatemala]. Since 1976, killings have included 68 cooperative leaders in the Ixcán, 40 community leaders in Chajul, 28 in Cotzal, and 32 in Nebaj. In one community, the presidents of Catholic Action, Caritas and the local cooperative and community development committee, as well as five sacristans and four bilingual teachers, have been killed. (Davis and Hodson 1982, 47)

> Guatemalan deaths from political violence, 1976–77, according to the *World Handbook*: 0.

The most complete and most widely used cross-national longitudinal data set on political protest and violence is provided by the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983). A prodigious undertaking, providing annual events data for 1948–77, it is not good enough. The data are mortally flawed for Central America and (I suspect) for the rest of Latin America and the Third World more generally.

The primary source for the data set is the *New York Times Index*, supplemented for Latin America by *Keesing's Contemporary Archives*. Taylor and Jodice point out (p. 12) that *Keesing's* draws from nine basic sources (all European, with the exception of the *New York Times*), supplemented, for Latin America, with

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six additional sources—again, not located in Latin America itself.

Taylor and Jodice give some consideration to the accuracy of their data set, but this attention is minimal.[5] They do mention the alternative of utilizing national newspapers, which would, of course, be inordinately expensive (1983, 11, 181). Their defense rests largely on a study by Jackman and Boyd (1979) of the costs and benefits of using multiple sources to measure political conflict. Jackman and Boyd's sample is 30 African countries during the 1960–67 period.[6] Jackman and Boyd conclude that their "results provide good support of [the 1972 *Handbook*'s] claim that increasing coverage beyond two sources is not likely to result in substantial changes in the 'overall picture'" (p. 457).

Well, yes and no. It depends on whether *overall picture* refers to the accurate measurement of what happened in African (or Central American) country X or to the score obtained from using additional sources of the type already used for these data sets. Jackman and Boyd's "multiple sources" for Africa are (in addition to the primary source of the *New York Times Index*), *Keesing's*, *Facts on File*, *Africa Diary*, and *African Recorder*. The latter two are claimed as "regional sources," but both are published in New Dehli. None of their "multiple sources" are African.[7] It is not surprising, therefore, that Jackman and Boyd discover "that the cost–benefit ratio from using multiple sources may be unnecessarily high" (1979, 456).[8]

Obviously, cost factors cannot be ignored; it is certain that to incorporate truly regional—if not national—sources into the *Handbook* would present tremendous, and perhaps insurmountable, difficulties. However, the validity of the data set rests not on financial considerations but on accuracy. In response to criticisms of "imprecision" in the 1972 edition of this data set (as well as in others), Gurr has argued that "the kinds of inferences drawn in quantitative macropolitics are not likely to be affected by anything less than gross and systematic error" (1974, 250). And that is precisely the charge. If valid, this claim has serious implications. As Lichbach has noted, the *World Handbook*'s indicator has been settled upon by scholars as the common dependent variable in cross-national statistical studies of political conflict (1989, 451).

> On November 23, 1974, National Guard troops with tanks and bazookas attacked a group of peasants in La Cayetana, San Vicente, [El Salvador,] killing six and arresting twenty-six (later thirteen were found to have disappeared).[9] These peasants had occupied sixty *manzanas* of idle land belonging to an absentee owner, after repeated attempts to rent the land had failed. . . . By some accounts, the military met . . . and decided that to allow the land occupation would be setting a dangerous precedent, and so they decided to attack the area. (Berryman 1984, 110)

New York Times and *Keesing's*: event not reported.

The *Handbook* grossly understates Central American deaths from political violence. For example, standard estimates for deaths and disappearances from political violence in Guatemala for 1966–68 are 3 thousand to 8 thousand, with another 15 thousand for 1970–75 (Brockett 1988, 106–7); the *Handbook* gives only 207 deaths for the overall period.[10] Similarly, for Nicaragua, it has been estimated that over a thousand peasants were killed during the 33 months of martial law beginning at the end of 1974 (Brockett 1988, 168); the *Handbook* gives only 59 deaths for the encompassing five-year period. Although the death toll in El Salvador was far less for the period under examination than in the first two countries, those acquainted with Salvadoran events will agree that the 22 deaths reported by the *Handbook* for 1973–77 substantially underreports the tragic reality.[11] Even allowing for wild exaggerations in the "standard" estimates for these countries, the *Handbook* is clearly in gross error.[12]

Probably the most striking anomaly in the data set is that the *Handbook* reports political deaths in Honduras during the mid-1970s as twice as numerous as in either El Salvador or Guatemala! In reality, Honduras would rank fourth with Guatemala far ahead for all five. The unavoidable conclusion, then, is not just that the *Handbook* scores seriously underreport various countries but, more seriously, that a more accurate reporting would alter both the rankings between countries and the magnitude of the intervals between country scores, thereby substantially altering quantitative analyses utilizing this data set.

There are undoubtedly systematic factors at work producing the gross errors in the data reported by the *Handbook*'s sources. Political violence directed at peasants in remote sections of countries without permanently stationed *New York Times* correspondents is much less likely to be reported than urban violence in countries with permanent correspondents. Simply said, remote rural disappearances are harder to tabulate than assassinations in city streets. Other factors are at work, as well. The *Handbook*'s Honduran data for 1975 are surprisingly complete.[13] This was probably a coincidence, however, resulting from a systematic bias. The Honduran "bananagate" scandal of 1975 reached right into Wall Street, generating unparalleled coverage of Honduran affairs in the U.S. press.[14] As a result, the six people killed in June of that year at a peasant training center received mention by the *New York Times*, although the six peasants killed in 1972 in La Tlanquera were missed, their deaths having preceded the scandal.

Finally, there can be discrepancies between the *New York Times Index* and the news story itself:

> While a worldwide television audience saw El Salvador's sunny beaches before the "Miss Universe" finals July 19 [1975], off-camera heavily armed troops were called out to halt demonstrations by students protesting the Government's expenditure of $4-million on the contest. . . .
>
> Then on July 30 about 3,000 students demonstrating in San Salvador against repression of . . . earlier marches were stopped by machine-gun and automatic-rifle fire from soldiers.
>
> According to the military Government, which con-

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TABLE 1

Land Inequality and Disruption Potential

MEASURES OF LAND INEQUALITY	COSTA RICA	EL SALVADOR	GUATEMALA	HONDURAS	NICARAGUA
1. Gini index of land concentration	.81	.81	.82	.78	.80
2. Smallholders[a] as % of all operators	43	74	87	64	51
3. Average size of smallholder farm (hectares)	1.7	1.2	1.8	1.9	2.6
4. Minifundización score[b]	25	62	48	34	20
5. % of all land in largest farms[c]	54	64	69	56	54
6. Landless score	20	36	32	32	27
7. Inequality score[d]	99	162	149	122	101
8. % rural population	59	62	66	72	51
9. Relative rural disruption potential[e]	59	100	99	87	52

Sources:
Row 1: Muller and Seligson 1987, Table A-1 for early 1970s except Guatemala (1964) and Nicaragua (1963).
Rows 2–3: Muller, Seligson, and Fu 1989. Table 2 for early 1970s except Guatemala (1964) and Nicaragua (1963); substituting data for Guatemala 1979 and Nicaragua 1971 made little difference.
Row 5: Brockett 1988, Table 4.1; also United Nations, Food and Agriculture Organization 1981, Table 3.3; for further discussion, see my n. 20.
Row 6: Prosterman and Riedinger's data for early 1970s (as reported by Muller and Seligson 1987, Table A-1) does not include Honduras or Nicaragua; Prosterman and Riedinger's later efforts (1987, Table 2) are for the early 1980s (except for El Salvador and Nicaragua, which are for the period just prior to their reforms). The data used here are an estimate for the mid-1970s, using the midpoint of the two data sets (and the mid-point for the 1987 data set where a range was given). Mid-1970 scores for Honduras and Nicaragua were estimated by reducing the score for the later date by the same amount as the average of El Salvador and Guatemala (Costa Rica is less comparable).
Row 8: United Nations 1976, 171–72 for early 1970s.
Notes:
[a] Smallholders are those with five hectares or less.
[b] Row 2 divided by row 3.
[c] Amount of total land held by largest 3.5–4% of farms in early 1970s (1979 for Guatemala). Cut-off sizes in hectares: Costa Rica, 200; El Salvador, 20; Guatemala, 22.4/44.8; Honduras, 50; Nicaragua, 345/690.
[d] Sum of rows 4–6.
[e] Row 7 times row 8.

tended that the march was part of a "Communist plot," *one person was killed*, five wounded and 11 arrested. But according to the students, *at least 12 persons were killed*, 20 wounded and 40 arrested. Witnesses said that about 50 persons, some apparently dead and others bleeding, had been taken away in army ambulances and trucks.[15]

The index lists only the one death claimed by the Government in the July 30 shootings, ignoring the higher number reported by witnesses.

Nonetheless, if the Central American data for 1948–77 were to be corrected, the political violence scores of Honduras and El Salvador would probably group together when placed alongside the much higher score of Guatemala. This raises at least two major theoretical questions: (1) What is the relationship, if any, between the political violence rates of the past and the future? (the *Handbook*'s 30-year data set ends in 1977—just when violence in El Salvador escalated to the tragic level of Guatemala)[16] and (2) What do these country groupings mean for the purported relationship between land inequality and mass political violence? Was the agrarian structure of El Salvador more similar to that of Honduras than to Guatemala prior to 1977? Did El Salvador's rural inequality then change, precipitating the violence that has now persisted for more than a decade?

MEASURING LAND INEQUALITY

Land inequality is often just measured by the Gini index,[17] which is so simplistic and misleading that it is of limited utility for meaningful cross-national comparisons. Indeed, as Table 1 indicates, the Gini land inequality scores for the Central American countries are virtually identical. For such reasons, Midlarsky (1988, 1989) argues, instead, for a measure of "patterned inequality," by which he means a comparison of the pattern of concentration of land holdings between smallholders and largeholders. In turn, however, Muller, Seligson, and Fu convincingly demonstrate that Midlarsky's measure is "interesting conceptually but so flawed in construction as to be of little utility" (1989, 586).[18] They create instead an "index of bifurcated inequality." This measure is constructed from the proportion of small farms and from the average sizes of small farms compared to large farms.

Both sets of authors are attempting to measure what might be termed *minifundización*, that is, to devise a measure that captures the discontent of the smallholder without enough land to support a family yet confronts other farms that are of vast and "unjust" size. The Muller, Seligson, and Fu approach appears adequate for this purpose on the lower end

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of the land distribution scale. However, it would seem that what is most relevant to the smallholder about large estates is not their average size but rather the extent to which they dominate the local land supply. Beyond a certain size, an "unjustly" huge farm is just that—a large farm unjustly dominating the land supply, be it four hundred or four thousand hectares in size. Furthermore, the subdivision of large farms within the elite (by inheritance, sale, or subterfuge to avoid land reform laws) has no effect on land inequality from the perspective of the smallholder but could have a substantial impact on the bifurcation measure if the subdivisions were sufficiently prevalent. Accordingly, the share of total agricultural land dominated by the largest farms would be a better indicator of the role of the upper end of the land distribution scale in creating land inequality and peasant discontent than is the average size of "large" farms.[19]

Unlike the Gini index, these measures indicate substantial differences between the Central American countries. As row 2 of the table shows, *minifundios* are much more prevalent in Guatemala and El Salvador—in fact, twice as prevalent in the former than in Costa Rica. Similarly, the countries vary in the average size of the smallholder's land. Row 4 reports a score created by combining these two measures, which demonstrates *minifundización* to be the worst in El Salvador, followed by Guatemala and then Honduras, with Costa Rica and then Nicaragua grouped at the bottom. At the top end of the land distribution scale, essentially the same rankings obtain: Guatemala is closely followed by El Salvador, with the greatest concentration of land in the largest estates, and Nicaragua and Costa Rica with the least.[20]

Land distribution data are important indicators of rural inequality but are insufficient by themselves because of the increasing prevalence of landlessness among peasants. In fact, the landless of Central America constitute about one-third of the total population (prior to the agrarian reforms of the 1980s), with the exception of Costa Rica. Prosterman and Riedinger (1987, 10–11, 25) define landlessness as the lack of "ownership or ownership-like rights" among "those who cultivate the land." The landless include tenants, since they lack secure use of the land and face landlord extractions, amounts over which they usually have little control. The figures reported in Table 1, however, mask significant differences in the situation of the landless. Tenants vary widely in the security of their tenancy and the income-generating potential of "their" land. It is probable, for example, that the higher landless score of Honduras is less a source of intense discontent than the lower score of Guatemala, where the seasonal migration of Indians from the highlands to work on plantations was the largest migratory-labor-force-as-a-percentage-of-total-population in the world during the 1970s—and under extraordinarily oppressive circumstances (Paige 1975, 361).

These various indicators can be combined into an overall measure of land inequality. Row 7 reports such an inequality score, which combines, with equal weight, the three dimensions: *minifundización*, land concentration in the largest estates, and landlessness. By this measure, the worst land inequality in the early 1970s in Central America was found in El Salvador and then Guatemala, with a larger gap between them and Honduras, followed (by a large gap, again) with Nicaragua and Costa Rica. If land inequality translates into political action and conflict, it might be argued that the relative size of the rural population could be an important factor. Accordingly, row 9 takes this factor into consideration, giving a score for "the relative rural disruption potential" (with largely the same country rankings but a closing of the gap between the top three countries). Regardless of which score is used, the essential point is that a multidimensional measurement of land inequality demonstrates substantial differences between the five countries of Central America.

Devising adequate measures of land inequality, however, takes us only a short distance, since it is unlikely that there is any direct relationship between land inequality and political violence. A major weakness of statistical analyses of comparative political violence is that they are largely devoid of any theoretical link between macro-economic processes and structures and either individual or collective behavior.[21] For example, much of the case literature suggests that changes for the worse in economic status are more likely to lead to discontent and violence than would a constant misery.[22] It is conceivable, then, that countries and regions with high but stable levels of land inequality might be less prone to rural unrest than would others with lower levels of land inequality but where the economic security of the peasantry is markedly deteriorating.

BRINGING IN SOCIAL COMPLEXITY

To make the point more concretely, imagine two countries with similar land distribution patterns and landlessness rates. In country A, the agrarian system has remained fairly stable for decades; but high population growth rates have steadily driven up the levels of landlessness. Country B also experiences population growth; but, in addition, the commercialization of agriculture has rapidly transformed its agrarian system. Profit-motivated commercial farmers in country B use their superior resources (monetary and coercive) to dispossess peasants of lands to which they had long enjoyed ownershiplike use and to break long-term tenancy relationships. In both countries there will be mass poverty, but deprivation alone seldom leads directly to mass mobilization. In country B, which is the superior approximation of the Central American reality, discontent has a target.[23] It is not so much poverty alone, Prosterman and Riedinger point out, "as it is *blamable* poverty that seems to serve as a predictor of violence" (1987, 9). When the disadvantaged live in dangerous political environments, assertion of their demands is risky. To

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overcome this discouraging environment, mass collective action usually requires "not only a sense of misery but also a sense of outrage and injustice" (Huntington and Nelson 1976, 102).[24] A good illustration of this point is provided by White's explanation of peasant mobilization in Honduras:

> The brutality of many of these evictions [of semisubsistence farmers by larger landholders] proved to be the catalyst in breaking down the friendly dependence on helpful patrons and developed a profoundly emotional opposition. The evictions were the sudden, sharp deprivation which moved campesinos to risk their lives in organizing to counter rural elites and protest before government authorities. . . . It was the small farmers who became the leaders in the national mobilization of campesinos. The evictions or other means of blocking access to land to which campesinos thought they had a legitimate right also became the basis for the land occupations or recuperations (1977, 181–82).

A final difficulty in assessing the relationship between land inequality and mass political mobilization and violence is the temporal dimension of this relationship. Do changes in the level of rural discontent resulting from growing inequality promptly cause corresponding changes in the level of mobilization and/or violence or might that effect be delayed? More concretely, high levels of peasant mobilization and resulting rural conflict were reached in Honduras in the late 1960s, about half a decade prior to similar mobilization and conflict in Guatemala, itself preceding those of El Salvador and then Nicaragua.[25] To what extent do these country rankings correlate with differences in levels of land inequality?

Although critical, the temporal dimension of the relationship between land inequality and political violence has received little theoretical attention. Midlarsky, for example, ignores the question except for a passing reference to the "generally slow" development of antiregime organizations (1986, 498–99).[26] If there is a long lag between the generation of inequality-based discontent and of social mobilization (and, therefore, political conflict and violence), then the correlation between inequality and violence is confounded (and many data analyses, as well).[27] If we postulate that there is a long lag, that is, that country C and country D have the same level of inequality, but that the inequality is of more recent origin in country D, then it follows that we would expect lower levels of violence in country D. However, why should we even hypothesize a temporal gap between the origins of discontent and of political mobilization and/or violence? And might whatever gap that does exist vary between countries because of the influence of other variables?

To return to the example of Central America and the development of high levels of peasant mobilization in Honduras prior to other countries—in actuality, the postwar agrarian transformation accelerated *last* in Honduras; that is to say, in Honduras there was little delay in time between the generation of discontent based on increasing land inequality and peasant mobilization and political conflict—certainly less of a gap than in the other countries. In contrast, El Salvador entered the postwar period with the most unequal agrarian system of the five; but its rural mobilization lagged behind that of Honduras, and its political violence rate was far below that of Guatemala until the end of the 1970s.

The countries of Central America, then, provide wide variations in the temporal relationship between land inequality and violence. The critical intervening variable that explains these variations is the political context. Largely ignored in earlier research, the critical conditioning role of political variables has fortunately received growing emphasis in recent studies.[28] Although too broad a subject to include here, the inclusion of the political context would explain the confusing relationships between land inequality and political violence found in the Central American cases, such as the very different relationship between the two variables for El Salvador and Honduras, as well as high enough levels of political violence in Nicaragua in the last third of the 1970s to place it with El Salvador and Guatemala, when its land inequality score was more like that of Costa Rica.[29]

CONCLUSION

This research note has discussed a number of hypotheses concerning the relationship between land inequality and mass political violence. Empirical social science teaches well the importance of rigorous hypothesis testing with the appropriate evidence. Regrettably, it sometimes unintentionally reminds us of the necessity of ensuring the validity of the empirical data utilized for such testing. If the *World Handbook*'s data set provided reasonably accurate measurements of the cross-national levels of mass political violence, it would allow for the meaningful testing of the relationships I have examined. However, the *World Handbook* cannot be used for this purpose. The data for at least Central America are grossly inaccurate. Since the data set is not valid for this region, the burden of proof falls on future users to demonstrate that it is sufficiently accurate for the regions of their study. The appropriateness of this data set for cross-national studies that wish to generalize globally, however, is another matter. Since at least part of the *World Handbook*'s data is the product of gross and systematic error, valid cross-national statistical tests of global hypotheses and theoretical models concerning political violence must await the construction of better data sets.

The compilation of the *World Handbook*'s data set on political protest and violence was an extraordinary undertaking and accomplishment; but by the standard of accuracy, it was an unjustifiable shortcut for Central America and (probably) for the rest of the Third World. The construction of reasonably accurate data sets for these countries will probably have to be done on the basis of a substantial division of labor, with small regions researched by different scholars. Instructive for these future efforts would be Tarrow's

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(1989) use of national newspapers for his study of Italy and Tilly, Tilly, and Tilly's (1975) use of truly multiple sources in their comparative study of France, Italy, and Germany. Of course, as we move from institutionalized democracies to most Third World countries, censorship becomes a major obstacle to the development of accurate data sets.[30] A notable attempt to address this problem for a Central American country through the use of both national and international sources is the chronology of political violence in the Guatemalan highlands for 1976–82 compiled by Davis and Hodson (1982). They utilized five Guatemalan and four Mexican newspapers; one Costa Rican newspaper; North American newspapers and magazines; and reports from human rights, governmental, and religious organizations.

Future scholarship also must take social complexity into account better than heretofore. First, land inequality is multidimensional, involving, at a minimum, landlessness, insufficient land, and land concentration. Second, a more general concept of *rural inequality* would add to these dimensions other factors, such as income inequality and the security of tenancy and paid labor arrangements. Third, subjective experience is critical to mobilization. Similar levels of inequality can correlate with various levels of political mobilization and violence because of differences in the perception of the sources of that inequality. The available case material suggests that the most explosive situations arise when peasants believe they have been "unjustly" dispossessed of land. Finally, there is no direct relationship between rural discontent based on the perception of unjust inequality with levels of peasant mobilization, political conflict, and political violence. Instead, this relationship is mediated by the always-changing political context.

Notes

An earlier version of this study was presented at the fifteenth international congress of the Latin American Studies Association, Miami, 1989. I thank Mitchell Seligson for his helpful comments.

1. Midlarsky (1988, 503–4) appropriately objects that the Gini index is an invalid measure of what I have called here the *minifundización* hypothesis, of which his "patterned inequality" hypothesis is an example. Additional objections to the Gini index are provided by Prosterman and Riedinger (1987, 24–25). Midlarsky's debate with Muller and Seligson continued in more recent publications.

2. The underlying arguments vary from the inherent value of stability, to reform as preemptive of more radical movements and changes, to stability as the by-product of doing what is just (i.e., reallocating to peasants what they deserve).

3. These two studies are among the best of those that have examined the relationship between economic inequality and political conflict. For critical reviews of this literature, both highlighting the disappointing lack of theoretical progress, see Zimmerman 1980 and, almost a decade later, Lichbach 1989.

4. Although the focus here will remain on land inequality, a fuller account of the sources of rural violence would include other dimensions of rural inequality, such as income. There is, of course, a thriving literature on the socioeconomic determinants of rural collective action, among them McClintock 1984, Migdal 1984, Paige 1975, Popkin 1979, Scott 1976, Tutino 1986, and Wolf 1969.

5. Earlier editions of the *Handbook* were more tentative about the data set's accuracy (Russett et al. 1964, 97–98; Taylor and Hudson 1972, 64–65, 417–423).

6. Jackman (1978) uses this data for a quantitative analysis of the determinants of African coups.

7. The great preponderance of the *African Recorder*'s sources are Indian and Western European (particularly British). A quick check of the first four issues of 1962 did find some reports from Africa-based newspapers, all from Nairobi (except one from Dar es Salaam). The first four issues of 1988 were even more European-based. In a sample of six scattered 1961 issues of the *Africa Diary* close to half of the sources were African, but 61% of these were from two newspapers, one in Accra, the other in Nairobi. In addition, with the exception of Ghana and Kenya, coverage ran heavily to official travels and government announcements. For both sources for this analysis, Northern Africa and South Africa were ignored.

8. An additional deficiency of Jackman and Boyd's study is that they test only whether source differences affect hypothesis testing, but, examination at this gross level of aggregation could mask wide variations in country scores between sources. Precisely this problem had been demonstrated earlier by Doran, Pendley, and Antunes (1973); but Jackman and Boyd do not address this aspect of the earlier study. The Jackman and Boyd study neither compares its five sources for variations in their annual scores for a particular country nor, more importantly, does it compare differences between these five sources and other, more authoritative, sources. For example, while the relevant *Handbook* score mirrors the Burundi massacre of 1972, its report of 106 deaths in Burundi for 1963–67 obviously misses the massacre of some 2,500 to 5,000 Hutu in 1965 (Kuper 1981, 63).

9. The names of the murdered and of 10 of the disappeared can be found in the congressional testimony of Fabio Castillo, former president of the National University of El Salvador (U.S. House, Committee on International Relations 1976, 41).

10. A small story reporting the Amnesty International charge of some twenty thousand deaths and disappearances in Guatemala during the prior decade appeared in the *New York Times* on 12 December 1976. Although it is obvious that this kind of data could not be included in the events data set, it is curious that it did not lead to a reexamination of the validity of the Guatemalan data compiled for the *Handbook*.

11. To his credit, Midlarsky (1988) realizes that there are serious problems with the Central American data. His "solution," however, is inadequate. He uses the entire 1948–77 data set, dividing the Latin American countries into quintiles. However, because "El Salvador and Nicaragua . . . are known to have experienced intense violence in this period," they are artificially moved into the top quintile (El Salvador all the way from the bottom). Guatemala, where the most deaths actually occurred, is left in the third quintile (when it belongs in the first); nor are the scores of any of the other countries reconsidered. In response to the excellent critique by Muller, Seligson, and Fu (1989) of his handling of the dependent variable, Midlarsky (1989, 589) refers to the "arguments developed" in note 7 of his 1988 article. These "arguments" however, consist solely of a quick reference to LaFeber's *Inevitable Revolutions* (1983). The cited page in LaFeber makes no reference to El Salvador at all; its discussion of Nicaragua is in terms of the tens of thousands of deaths in the war against Somoza, almost all of which occurred after 1977, that is, after the end date of the data set.

12. If the data are erroneous for one region, similar problems would presumably be found elsewhere as well. Just a quick glance at the annual country scores shows the following examples of underreporting of political deaths: Chile 1973, 537 deaths; Brazil 1968–72, 36; and Uruguay 1973–77, 14.

13. The data for 1976, however, are another matter. The *Handbook* reports 15 Honduran deaths for 1976. The only relevant *New York Times* story (24 July, 1976) lists 14 deaths—but of Salvadoran soldiers killed in a border conflict with Honduran armed forces. The *Keesing's* edition of 30 April,

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1976 (p. 27708) does list 15 peasants killed in Honduras; however, the deaths are reported as having occurred on 8 November 1975 (and are already included in the *Handbook* total for 1975). In fact, the process by which *Keesing's* was utilized by the compilers of the *Handbook*'s scores is a mystery. For example, the *Handbook* reports 22 deaths for Guatemala in 1973; the *New York Times*, 17; so *Keesing's* (6–12 August, 1973, p. 26025) total of 22 must have been utilized. Yet in the following year *Keesing's* (18–24 March, 1974, p. 26419) mentions 8 Guatemalan deaths but the *Handbook* only lists 1. Similarly, the *Handbook*'s total for El Salvador during 1974–75 is only 3 political killings but *Keesing's* (10 September, 1976, p. 27938) lists 26 killings just for November 1974–September 1975.

14. The *New York Times'* first 1975 story on Honduras appeared on 10 April with the U.S. Security and Exchange Commission's charge that United Brands (the successor to United Fruit) had paid a $1.25-million bribe to the Honduran president to obtain favorable tax treatment on its banana exports. In April and May, the *Times* ran some 24 stories related to Honduras, all involving the scandal and its aftermath. The rest of the year had 17 more stories, including several concerning the escalating rural unrest in the country. During the previous three years the *Times* had run only nine stories on Honduras, none of which involved the considerable unrest in the countryside. Meanwhile in 1975, the rest of Central America was largely ignored in the *Times*. For El Salvador there were but three stories and for Guatemala only one. Costa Rica did have nine articles; but five were scandals related to the United States, and the remainder were short pieces. Similarly, four of Nicaragua's seven stories ran in January following the audacious Christmas party attack by the Sandinistas; and two of the remainder concerned the new U.S. ambassador.

15. "Unrest Growing in El Salvador" *New York Times*, 10 August 1975 (emphasis added). Other sources give a higher death toll—for example, Berryman at least 20 dead (1984, 111); Montgomery at least 37 dead and "several dozen" more disappeared (1982, 89), Webre at least 37 dead and "many more" disappeared (1979, 189).

16. One presumes that it was this juxtaposition that led Midlarsky (1988) to arbitrarily alter El Salvador's place in his ranking of the Latin American countries on his political violence scale.

17. Muller and Seligson, for example, use the Gini index for measuring inequality in the distribution of land, which is then combined with the proportion of the labor force employed in the agricultural sector to yield an "agrarian inequality" score (1987, 435).

18. Indeed, this statement can be aptly applied to the whole study. Most of its weaknesses are identified by Muller, Seligson, and Fu (1989). Although still critical, Lichbach sees more merit in Midlarsky's work, praising it as "the most sophisticated and relevant formal models of the [economic inequality–political conflict] nexus" (1989, 452).

19. Another advantage is that this approach avoids the impossible task of deciding what constitutes a "large" farm. For a discussion of these difficulties, see Muller, Seligson, and Fu 1989, 580–81.

20. The decision of how to operationalize "the largest farms" was an artifact of the data sources. The available land distribution data are reported by size categories, whose number varies between countries but usually around 10–12. The procedure was to find breaks between categories that yielded approximately the same cutoff point for "the largest farms" for each country (which turned out to be between the largest 3½–4% of all farms). This could be done for three of the countries, but the percentage of land held by the largest farms had to be extrapolated for two (Guatemala and Nicaragua). As note c to Table 1 indicates, the cutoff point in hectares varied substantially between countries—another manifestation of the large difference between them in the extent of land concentration.

21. Lichbach discusses this deficiency insightfully (1989, 448–55).

22. Eckstein is a recent example of the argument that "economic relationships, especially changing economic relationships [are] the principal cause of protest and pressure for change" (1989, 4; also 16–17).

23. Midlarsky accounts for *minifundización* almost entirely in terms of population growth (1988, 494–95). He gives virtually no consideration to the other factors discussed for country B, which scholars have identified as crucial to accounting for the political conflict in the region (Brockett 1988; Williams 1986).

24. Similarly, see Gurr 1970, 155–231; Jenkins 1985, 5–6; McAdam 1982, 34; Moore 1978, 459–71; and Scott 1977, 236.

25. These generalizations are not based on a quantitative ranking but, rather, a familiarity with the relevant literature (see Brockett 1988). This ranking disregards state terrorism directed at largely unorganized peasants as part of counterinsurgency campaigns (i.e., Guatemala in the mid-1960s and Nicaragua in the mid-1970s).

26. This hypothesis has no bearing on Midlarsky's empirical test, which completely overlooks the temporal relationship of his variables, thereby confusing the causal connection between them. His dependent variable is the total number of deaths from political violence for 1948–77 (as compiled by the *Political Handbook*). His independent variable is constructed from the earliest data available on land concentration patterns. For Costa Rica, the date of the independent variable data comes after virtually all of the political killings; for Honduras and Nicaragua, it is approximately simultaneous with the date of the midpoint of the killings; while for El Salvador and Guatemala, the date of the independent variable data precedes most of the killings. What, then, is the theoretical relationship between the two variables? Muller and Seligson (1987) are better; their dependent variable is for 1973–77, and they try to get their independent variable data from as close to 1970 as possible.

27. In a commentary on an earlier study by Muller, Weede notes the absence in the literature of any "theoretical defense of lagging or not lagging" (1986, 438). Also see the more general, and still relevant, theoretical critique by Gurr and Duvall (1973, 136).

28. In addition to Muller and Seligson 1987, among the best examples are McClintock 1984 and Midlarsky and Roberts 1985.

29. For an explanation of these differences, see Brockett 1991.

30. Illustrative of the problem is Lindenberg's discovery concerning social discontent coverage in Central American newspapers: "Precisely at the moments when historical sources indicate that there was the most discontent, evidence of the volume of that discontent disappears in countries whose newspapers were censored." (1990, 405).

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POLITICAL OBLIGATION, FREEDOM AND FEMINISM

How should feminist theorists approach the analysis of political obligation and freedom? In her article in the December 1989 issue of this Review, *Nancy Hirschmann proposed a fundamental reconstruction of these concepts. Carole Pateman agrees that liberal obligation theory inadequately treats the coercion of women and other groups under the guise of voluntary obligations. But she argues that Hirschmann goes too far, particularly in rejecting the priority of freedom. Hirschmann explicates their disagreements and her approach.*

Sinopoli and Hirschmann's (1991) controversy in the *Review* unfortunately fails to confront Hirschmann's (1989) major claim that the idea of voluntary, or self-assumed, obligation and the priority thus given to freedom is a serious problem for feminists. She concludes that obligation must be reformulated in terms of involuntary, nonconsensual, or "given" social relations and put into a context of connection and responsibility.

I agree with Hirschmann about the inadequacy of contemporary theories of political obligation and their patriarchal character and endorse her call for a new approach and new questions, but I want to take issue with her discussion of political obligation and her conclusion. I will not examine object relations theory, although I do not believe that this provides an appropriate new approach (see e.g. Young 1984) because, as I shall indicate, the case can be made that standard arguments about political obligation are patriarchal from a consideration of classic and contemporary arguments.

OBLIGATION

Hirschmann argues that critics (including myself) of what she calls "consent theory" have failed to look deeply enough at the well-known problems with the theory; the symptoms, not the causes have been addressed. The critics take it for granted that obligation is self-assumed, and they have not asked why abstract individualism is so central to consent theory.

A major problem in Hirschmann's discussion is that she assimilates *obligation* to *consent*. She states, for example, that I argue for a "fully consistent consent theory" (1989, 1228) when I took considerable pains to distinguish my argument about self-assumed obligation from arguments about consent (Pateman 1985). I may not have succeeded but Hirschmann does not show this. The idea of self-assumed obligation is important because to enter an obligation—to commit oneself—is to create a new social relationship. The idea is also very radical, because it depends on the assumption that individuals are free and equal to each other. The only way, therefore, that this assumption can be maintained and legitimate government created is through free agreement, or the voluntary creation of such an arrangement. The idea of self-assumed obligation thus turned the justification of modern government, whether in the state, the plantation, or the workplace or of men over women, into a general and permanent problem.

In contrast, consent must be given *to* something. It is precisely "consent theory" that treats obligation as already established or given. Indeed, at one point Hirschmann writes of "the necessity of consent to obligation" (1989, 1234). In the case of theories about original contracts, consent is (said to be) given by the next generation of citizens to the political obligation created through the original agreement. In contemporary theory, as in classic contract theory, the problem is taken to be to show how consent is given or, why there are reasons for obedience, rather than to show how political obligation is created and recreated or whether it exists. The prevailing assumptions by political theorists are that political obligation exists and consent is always given, thus, a variety of hypotheses are advanced to explain how this takes place; that is, self-assumed obligation is turned into what I called hypothetical voluntarism. The *problem* about political obligation is conjured away (see Pateman 1985). Ironically, Hirschmann presents as a feminist alternative the "given" obligation that is central to almost all contemporary consent theory.

Hirschmann might argue that even hypothetical voluntarism is still voluntarism, and that is the problem. Hypothetical voluntarism still is an example of the "fear and refusal" of consent theory to which she refers in her reply to Sinopoli, to admit "nonconsensual obligations." She argues that consent theory has segregated nonconsensual obligations in the private sphere, assigned them to various groups, "women, workers, slaves", and then denied those groups a political voice (Sinopoli and Hirschmann 1991, 228). I am not clear whether Hirschmann is arguing here that consent theory ignores these groups or that they actually have nonconsensual obligations. Such groups have been incorporated into voluntarist "consent theory." Men become "workers" by entering into, or consenting to, the employment contract, women become "wives" by entering into the marriage contract, and it has even been argued by some classic contract theorists and defenders of slavery in the Old South that "slaves" are constituted through a contract. To be sure, socialists and feminists have argued that the marriage and employment contracts are not valid contracts—that workers and wives, like slaves, are coerced. Such an argument implies that

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consequently there are no "obligations", merely enforced obedience. However, the status of Hirschmann's nonconsensual obligations is not clear. She mentions "unfair bargaining positions," for example (1989, 1239), but what does this imply for her major claim that obligations are given and nonconsensual? Are they valid? Her answer in the case of mothers' given obligation to care for their children (the focus of her argument) is equivocal (p. 1240). But this avoids the question whether "obligation" is the appropriate term, in this case, or in the case of workers and slaves, or whether other terms should be used.

Hirschmann notes that she does not discuss "duty" (ibid., 1243); but she does not mention "ought" or "obedience" either. "Nonconsensual obligation" could refer, depending on the context, to a variety of social relations. For instance, such "obligations" might be an example of enforced obedience. Or "duty" might be the relevant concept: we do not choose our duties, they come with our involuntary stations in life. Again, "nonconsensual obligation" could refer to actions that we ought to perform or from which we should forbear or point to the fact that there are others, at home or far away, for whom we should care. We may be forced to do something, or morally we ought to do it—but we do not have an *obligation* to do so unless we have made an explicit commitment. In colloquial speech "obligation" is often used broadly to cover "ought" and "duty", but that is no reason for political theorists to fail to distinguish them. To assume an obligation takes an individual beyond a personal moral judgment (I ought to do [not do] X) to a public commitment to action (forbearance) and into a new relationship and responsibility.

ABSTRACT INDIVIDUALISM

Hirschmann's argument presupposes that the concept of self-assumed obligation depends on abstract individualism. On the contrary, abstract individualism prevents an adequate understanding of obligation and, in its most radical form (as Hobbes demonstrated), makes obligation impossible. Individuals seen in abstraction from each other and their social relations can necessarily make judgments only from a subjective, self-interested perspective, and obligation must be seen as a series of discrete acts. Thus, abstract individualism generates an endemic problem about entering and keeping commitments, best known in the formulation of the "problem" of promising.

To assume an obligation is to commit oneself (promise). But why should promises be kept? From an abstractly individualist viewpoint there is no good reason at all; self-interest will dictate that any promise might be broken. From an alternative perspective that might be called social individuality, there is no general problem about obligation (which is not to say that no questions arise about assuming and fulfilling obligations). Self-assumed obligation is a social practice and individuals can create obligations only because any single promise presupposes that obligations bind. To say "I promise" or the equivalent would be meaningless if there were not an intersubjective understanding that to say these words is to commit oneself (see Pateman 1985, ch. 2).[1] The practice of self-assumed obligation therefore entails the social relations, connection, responsibility, and, I would add trust, stressed by Hirschmann.

This argument does not answer Hirschmann's question why abstract individualism is important in theories of political obligation. Instead, it illustrates the kind of problems generated by widespread adherence to this standpoint. However, like Sinopoli (Sinopoli and Hirschmann 1991, 222), I am not clear exactly what kind of question Hirschmann is asking—whether about cause or meaning. She charges that critics of consent theory have not investigated the cause of abstract individualism. Her own argument about mother-only childrearing, and masculine "reactive autonomy" appears to be causal. Hirschmann claims that reactive autonomy "justifies—indeed, creates and perpetuates—the radical and abstract individualism of liberal democratic theory, the market model of society, a theory that obligation can exist only by virtue of voluntary assumption" (1989, 1237).

One difficulty with this approach is that some great critics of abstract individualism—namely, Rousseau, Hegel, and Marx—either explicitly excluded women from citizenship or failed to break free from patriarchal theory.[2] More importantly, if Hirschmann wishes to give an account of patriarchy as "constituted by the cultural, social, political, sexual, economic and interpersonal structures of the modern liberal era" (Sinopoli and Hirschmann 1991, 226), then I find it hard to see why a causal question about abstract individualism is appropriate or why mothers' caring for their children should bear the weight of explanation.

Indeed, in her reply to Sinopoli Hirschmann appears to draw back from causal argument, treating object relations theory as interpretive (ibid.,) and stating that she is "build[ing] an account" that understands "a world of meaning that allows only certain ideas and views to emerge" (p. 229). Thus, she seems to be asking how "the individual" is interpreted, or what meaning is given to the "individual" and the "self" in the modern liberal era. An elucidation of this meaning(s) does not require a discussion of mothering. To imagine that individuals could be seen as if they were separate from their relations and institutions was a brillant feat of theoretical innovation. This development occurred as the "old world" of tradition and natural, ascribed hierarchy was giving way to the "new world" of universalism, equality, and freedom, the world of the market and constitutional government. For the first time "individuals" appeared, outside of any network of duties and obedience; to be a "free individual" meant that the old constraints were swept away.

Theorists were faced with the new, general problem about power and government generated by the

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premise of individual freedom and equality. If the problem was to be circumvented and individuals were to (be said to) consent to modern government—or to make the point another way—if new constraints were to be instituted, the advantages of (modern) government had to be made clear. Here, of course, theorists used the device of the state of nature in which its inhabitants were pictured as abstracted, to a greater or lesser degree, from the bonds of social life (from the "old world") and subject to insecurity or even war. Hence, a "new world" was needed. Reason then dictated that they should enter into the original contract and continue, as subsequent generations should also continue, to give consent. Abstract individualism remains influential, I would suggest, because it enables theorists to avoid asking critical questions about social structures and to assume that political obligation is "given." All that remains is to show how consent can be said to take place.

Stories about original contracts describe the creation of a new, free political order, or civil society, in which individuals become citizens. The "individuals" in question, as Hirschmann stresses, are masculine. Sexual difference, as constructed by the classic contract theorists, is a political difference—the difference between freedom and subordination. Their picture of manhood and womanhood is a political construct, albeit that they use the language of nature. Only men, therefore, can create the new body politic and ties of political obligation. They establish the modern, "free" patriarchal order. (For details of the preceding argument, see Pateman 1988). The classic contract theorists had reason to attack the divine right of kings, despotism in the state, and claims by patriarchal theorists that kings and fathers were one and the same (the "old world"). They had no wish, however, to overthrow men's jurisdiction over women; rather, they had a reason to modernize patriarchy because they had vital interests at stake *as men*.

Does this mean that women are, therefore, excluded from the new political order? Hirschmann's answer appears to be that they are, but the implications of her argument for women's citizenship are unclear.

CITIZENSHIP

The conventional subject matter under the heading *political obligation* is the relation between citizens and state. Hirschmann (1989) discusses some contemporary theorists of political obligation, and subsequently states, "My focus is political obligation" (Sinopoli and Hirschmann 1991, 228). Her argument about women, though, is not about women's obligation as citizens but women's obligation in the family, especially as mothers. Feminists may well argue that this is a political question, or that women's public standing as citizens cannot be understood without taking account of their private status, but Hirschmann does not spell out how the two are connected. She remarks, "the point of my original article [was to] highlight women's systematic exclusion from political obligation theory" (p. 229; see also p. 226), but what is the connection between theory, women's duties as mothers, and their citizenship in practice? She also comments that she is not "suggesting that we model political obligations on the relationship between mother and child" (1989, 1242), but this still leaves open the question of how motherhood; citizenship; and women's political obligation, duty, or obedience are connected.

An emphasis on exclusion obscures the fact that the political *inclusion* of women is at issue too. Classic contract theory is about women's inclusion into the political order as men's subordinates, as well as their exclusion from citizenship. Exclusion from citizenship did not mean that women were excluded as political subjects or that they had no political duties, even though women's inclusion is not discussed by political theorists. Contemporary theorists assume that their arguments can apply to women or men (many of them now carefully add "she" even where completely inappropriate), but they do not ask whether men's and women's political incorporation has taken the same form or, now that women have won virtual equality in formal rights with men, whether the meaning of citizenship is the same for both sexes.

For instance, theorists of political obligation never look to see if there has been a vehicle for women's political inclusion that corresponds to that of "soldier" or "worker" for men. In some recent work, I have begun to explore "motherhood" as a political status and as a means of political incorporation from arguments about Republican motherhood in the United States to arguments and policies about population decline, eugenics and abortion and controversies about the welfare state (see Pateman 1992). Political theorists claim that the ultimate duty of citizens is to be willing, if the state so demands, to risk or give up their lives as soldiers. They do not stop to consider the significance that until very recently only men have been called as soldiers, or to consider whether an analagous duty to risk their lives has been demanded of women—in bearing babies to provide the population and future citizens of the state. "Motherhood", in this sense, does not mean relations between mothers and children; rather, it refers to the political tasks, service, and contribution of women that can be compared to the political contribution of men as workers or soldiers. The paradox is that motherhood has been constructed as quintessentially private, so that women's political duty of motherhood has been seen as opposed to citizenship. This construction has been challenged by feminists, many of whom have demanded, at least since the 1790s, that motherhood, as a service to the state, should be part of citizenship.

Hirschmann states that the "contingent gender bias of voluntarist obligation"—that is, cultural bias and practices that deny women opportunities for consent, and exclude women from standard means of consent (1989, 1229)—treats women as "incapable of consent." Even if a capacity for consent is granted,

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women are excluded because their experiences are denied (p. 1240). Certainly, women's experiences are denied political significance by political theorists; the problem is that this denial takes place in a context where women are included in (not excluded from) the "standard" practices of consent, whether these are part of marriage, sexual relations, voting, or other activities typically held to indicate consent.[3]

FREEDOM

Hirschmann argues that women's exclusion derives from the priority given to negative liberty or reactive autonomy in obligation theory. A feminist theory, therefore, would not give freedom priority (1989, 1241). Such a conclusion depends on the identification of freedom with absolute, negative freedom and a view of positive liberty as coercive (p. 1234).[4] Hirschmann writes, for example, that consent (i.e., obligation) "rests on a concept of liberty as absence of restraint" (ibid.). This argument entails that any bond created when an individual assumes an obligation is a restraint on freedom, but, as I have argued, this is to misunderstand obligation—and its connection to freedom.

Sinopoli criticises Hirschmann for an overly atomistic reading of Locke (Sinopoli and Hirschmann 1991, 224), but the criticism of her reading of the classic texts can be taken further. She writes of "the absolute natural freedom of the state of nature outlined by the social contract theorists" (1989, 1234), but this is true only of Hobbes. All the other classic contract theorists picture the natural condition as one of bonds and relations between the inhabitants, including conjugal and familial ties. Hirschmann also claims that since freedom is the "hallmark of humanity," women are not seen as human; that if they were, they would have a right to freedom (1989, 1236). I would not deny that doubts have been cast on women's human status by extreme misogynists and patriarchalists, but the classic contract theorists are hardly guilty of this. On the contrary, not only does Hobbes's portrayal of the natural condition include the striking presupposition that natural freedom and equality are the birthright of women as well as men—in the state of nature women are lords (see Pateman 1991)—but the other contract theorists have to admit women's freedom at the same time that they deny it.[5] In their states of nature, unlike Hobbes's, marriage exists and this "natural" relation is carried over into civil society. They all agree that marriage is based on a contract (this is true even of Hegel) and women are parties to this contract. Therefore, it follows that women share in freedom and the human status signified by "consent", notwithstanding that their "natural" subordination to men is institutionalized through the original contract (see Pateman 1988).

Hobbes showed that an abstract, absolute conception of freedom is no freedom at all. Self-assumed obligation presupposes a very different idea of freedom, a social conception in which freedom inheres in a certain form of sociopolitical relations (relations that include "oughts"). The idea of "obligation" enables individuals to do more than take these relations as given; it allows them both to reinforce interdependencies where appropriate and to create new bonds. Obligation is thus a means through which individuals and associations can exercise, maintain, and sometimes, depending on the character and content of the bonds, enhance their freedom.

Hirschmann states that "the purpose of a feminist approach to obligation is not to bind us more tightly to the state or to relations" (1989, 1242), but her argument about given "obligation" points in that direction. Liberal theory is not alone in giving priority to freedom; socialist theory has also shared that goal. Liberalism and socialism, however, have been patriarchal in theory and practice. Feminists need to do a good deal of theoretical reformulation, and I endeavored to contribute to that task in *The Sexual Contract*, where I looked at the construction of various dichotomies, notably public/private, that have been central to both the understanding of their subject matter by "political" theorists and the practice of contract involving property in the person. Feminists have no need to jetison the concept of obligation or, indeed, freedom, (there are a variety of nonnegative views available on which to draw), but theoretical work goes only so far; the structure of social and political institutions also has to be confronted. Contemporary theories of political obligation typically assume that a free political order exists, but this is not self-evidently the case.

Feminists would be ill advised to give up the priority of freedom, for no one else has given much heed to the problem of men's power and women's subordination. This is not to suggest that feminists should not be concerned with care, which Hirschmann sees as the feminist priority, but to draw attention to two questions. The first is whether the voice of care is always that of the powerless (see Sinopoli and Hirschmann 1991, 226). The powerful talk of *noblesse oblige*, and of the caring duties of "good masters" of all kinds, and are very accomplished at caring for each other. The second question is whether a political order in which all are cared for can be created if freedom for all is relinquished as a priority.

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I am grateful to Carole Pateman for providing this opportunity to expand on several complex points that had to be compressed in my original article (Hirschmann 1989) but that I take up in my book *Rethinking Obligation: A Feminist Method for Political Theory* (1992). My argument raised many controversial issues, not all of which could be addressed within the confines of a single article. Though I hope that my

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new book addresses these issues satisfactorily, Pateman's commentary raises important ideas that merit discussion in these pages.

There are many similarities between Pateman's work and my own. Indeed, many of the arguments she makes in her critique are at least guiding assumptions, if not explicit aspects, of my own argument. We are both concerned with political obligation, the social contract, and state legitimacy as they relate to the historical, political, ethical, and philosophical position of women. We both believe that there is a severe disjunction in liberal obligation theory between the language of voluntarism and the reality of coercion for women and other groups. We both emphasize participatory democracy as an important feature of a theory of political obligation. Finally, we both see a problematic relationship between the individual and society in modern obligation theory.

But whereas Pateman seems to want to refine that relationship, I am interested in questioning our understanding of it altogether by considering the epistemological framework that determines the meaning of the terms involved. This leads (as Pateman indicates) to disagreements between us about the priority of freedom and the meaning of consent. But these, I believe, are by-products of our strongest disagreement—over how best to conduct feminist analyses of political theory. Whereas Pateman's critique of patriarchy seeks to alter the particular construction of gender relationships within a liberal epistemological framework, I seek to analyze the framework itself and to identify the bias that exists in the epistemological underpinnings that define the modern world. The problematic relationships between individual and community, freedom and authority, and male power and female subjugation are not to be addressed by reorganizing the pieces on the board. Rather, the board itself must be reassessed and reconfigured before any such reorganization will have the desired effect.

CHOICE, CONSENT, AND GIVEN OBLIGATION

Pateman presents my argument as follows: women have traditionally been considered naturally bound to care for husbands, children, and others. But rather than seeing this historical bondage as problematic, we need to accept it as a new feminist starting point for thinking about politics. In short, she believes, my feminist theory of obligation could be taken to glorify women's oppression and exploitation and could give rise to a sort of feminist totalitarianism where individual difference and choice are suppressed in the interests of care.

But Pateman's misconstrual is obvious. In seeking to construct an alternate vision of obligation based on a feminist standpoint of connection and mutual recognition, I refer to women's history of care not as a simplistic endorsement of women's lack of choice but to understand why it is that *women* have historically lacked choice while men have defined and exercised freedom in the modern era. Furthermore, in the process, I show that this dichotomy between free men and bonded women is a constitutive element of modern understandings of the terms *freedom* and *obligation*; they demand women's continued exclusion from the class of "free" political actors, by definition.

As I particularly stressed, political obligation on the social contract model is flawed not merely because it gives an important role to individual choice per se, but rather for its artificial and oversimplified reification of choice, as well as of human relationship (Sinopoli and Hirschmann 1991, esp. 230). This dichotomy then carries over into the rest of human social relations: men are naturally free, women naturally unfree. But in order for women to gain greater freedom and choice, men must recognize their share in these unchosen obligations; and we must all recognize the role of such "private" activities in constituting the public realm.

Liberal obligation theory *tacitly* admits the importance of the nonconsensual obligations represented by women's experience. But it publicly denigrates them; and that is one of the purposes of its particularly modern version of the public/private split. That they *seem* unimportant to liberal theory stems not from their inherent value but from their bifurcated representation. By taking all of the aspects of life that cannot be *honestly* described as contractual, assigning them to women (even under the name of "duties," as if that answered, rather than begged, the question of choice), and then overtly disvaluing them, liberal theory can ensure that these aspects are taken care of without having to recognize the contradictions they pose for the theory. This, then, leaves men *ostensibly* free in the public sphere to choose (allegedly) all of their connections, relationships, and obligations.

Pateman (1979, 1985) is rightly critical of consent theory for its disingenuous elitism. She is attracted to a Rousseauist participatory democracy because it provides for more genuine individual participation and choice, since citizens have a role in deciding the content of their obligations—i.e. what they will consent *to*. But within the existing epistemological framework, this focus on equal citizens freely choosing offers a consistent rendering of the liberal ideal.[6] A totally voluntarist theory of obligation is possible only because of the existence of a class of people who take care of the nonconsensual obligations that characterize human life. Women's subjugation has endured precisely because of theorists' refusal to incorporate the aspects of life typified in women's experience; it is because of the theoretical contradictions they pose that women must be relegated to the private sphere and then devalued.

Pateman's desire to create the conditions that can provide for better choice is in itself admirable. I, too, am interested in creating such conditions, for women's choicelessness in history is pernicious and presents a serious contradiction for modern (liberal) politics. But the argument cannot stop there; to focus *only* on choice leads one into the trap of taking a reified masculinist experience as the norm that

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women should follow. Pateman does this, for instance, in calling the liberal abstraction of individuals from relationships and institutions "brilliant" (p. 180); for in reality, it was brilliant only for (white, bourgeois) men, since this notion of freedom and individualism vitally depended on the subjugation of large groups of other people, particularly women. That consent theory excludes women is not just happenstance (any more than it is conscious conspiracy) but a function of the epistemological structure of liberalism. It is only because liberalism has the public language of contract and can reify and separate noncontractual reality from the public discourse of political theory that the ideology is sustainable (Sinopoli and Hirschmann 1991, 228). And this emphasis on the public is possible only because of a repressed private realm, that is, a realm of noncontractual obligations assigned to women and then repressed or conveniently forgotten when liberal obligation theory is written. But when something is repressed, it is still present even though such presence is consciously denied.

This certainly does not mean that feminists need to glorify women's exploitation as caretakers or that we cannot critically assess and reject patriarchy. But even granting that feminists do not have obligations to the existing patriarchal context does not mean that women—any more than men—can break free of all contexts to drift in abstract freedom. Indeed, their greatest obligation may be to forge a new context—which in many ways is what feminism is all about.

Thus, my account of given obligation is hardly "the 'given' obligation that is central to almost all contemporary consent theory" (p. 179). The givenness of obligation in consent theory is covert, hidden, and even subverted to the end of an overt and explicit public ideology of choice and freedom.[7] My work suggests that theorists need to attend to the aspects of human existence that liberalism limits to the private sphere in order thereby to learn more about all kinds of obligation (including particularly political obligation), because "private" obligations constitute the public. The dichotomy between public and private obligation itself needs to be questioned. Pateman claims to challenge that dichotomy, but her remarks above actually endorse it. What could it mean to talk about "women's obligations as citizens" (which she opposes to "women's obligation in the family" [p. 181]) in the sixteenth century, for instance? Such political obligations, like women's private obligations, were of obedience only—moreover, of obedience *within the private sphere*, to husbands and fathers. Pateman oversimplifies the distinctions between obligation, duty, and obedience by ignoring how these terms are shuffled about and defined to suit patriarchal theoretical needs. To say that women were "included" in consent theory is thus misleading. It is more accurate to say that they are subsumed—and consumed—by it. Their agency is not only made invisible, but effectively disabled as well.

THE PRIORITY OF FREEDOM

Hence, at least *one* "connection between theory, women's duties as mothers, and their citizenship in practice" (as Pateman calls for, p. 181) is clear. Consent theorists do not want to explore the implications of "private" relationships for public ones, because this would require them to compromise the natural liberty they wish to claim for men. As I have argued twice already, theorists like Locke do not present as starkly negative a conception of liberty as Hobbes (Hirschmann 1989, 1233–34; Sinopoli and Hirschmann 1991, 228); yet the foundation of their consent theory is importantly negative. Pateman's claim that only Hobbes talks of "the absolute natural freedom of the state of nature" (p. 182) is far from true. Indeed, even though Rousseau embraces positive liberty in his ideal social contract, he is very similar to Hobbes in his description of the early stages of the state of nature, in that natural freedom is limited only by physical laws of nature (such as gravity) and eventually, as people come into more frequent contact with one another, by the relative strength of other individuals.

Locke himself casts the laws of nature as "limits" to natural freedom, which might suggest positive liberty (Hirschmann 1989, 1233–34). But while such laws are moral maxims (e.g., "Don't take more than you can use, because God did not give the earth for us to spoil"), they are also rational conclusions. For example, taking more than you can use wastes your labor (although once money is developed, it is rational to acquire more than you can use. Macpherson 1962). But for Locke, it is as wrong to talk about rationality "limiting" your freedom as it is to talk about gravity doing so: Why would anyone want to work for an entire day picking apples when half of them will spoil? God did not make men so imperfect. Or to the extent that he did and individual men violate or cannot access their God-given rationality, the social contract provides the remedy to make men do what they should want to do if they were rational enough. Hence, people who are not rational enough to see the need to consent to the social contract are said to do so tacitly. Locke's natural freedom is thus just as absolute as Hobbes's, even if the parameters of that freedom shift, and he binds people by as much of a "rational fiat" as does Hobbes (Hirschmann 1989, 1239). If the laws of nature were only moral maxims and Locke defined *freedom* positively, then why would we have to consent to government in the first place? Why would Locke not simply tell us what we should and should not do?[8]

Pateman's parallel claim that Hobbes is a sexual egalitarian is similarly problematic. While Hobbes argues that men and women are equal in the state of nature, women suddenly drop out of Hobbes's discourse in the formation of civil society. He does not even include them in his description of the family, let alone discuss their role as citizens (Hobbes 1968, 253). This contradiction reveals that his social contract is possible only if women become disempowered. In

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order to make sense of the fact that only men participate in the social contract, Pateman argues that the patriarchal nuclear family must predate the social contract (1988, 49); but she also notes the difficulty of this position. If women's childrearing created a temporary strategic disadvantage that enabled men to subjugate them, it is more likely to assume that infants would be abandoned by women. Faced with the choice of keeping a child and being subservient to a man or abandoning the child and maintaining her ability to fight men on an equal basis, Hobbesian woman should choose the latter.

Indeed, it would have made much more sense for Hobbes to declare that women consent equally to the social contract and then have the sovereign create patriarchy by commanding women to obey their husbands. But while this would be more consistent with *natural* sexual equality, it would also, paradoxically, make the structural sexism of Hobbes's *social* contract more apparent. (For a fuller discussion, see Hirschmann 1992, chap. 1 but also Di Stefano 1983). In light of this, Pateman's observation that Hobbes was raised by women is rather curious, because it supports my argument. As Di Stefano (1991) argues, Hobbes's life resonates strongly with an object-relational reading of his theory. It is, at least in part, because of the power of the mother that natural women appear monstrous; and it is because of the need to "escape the female" that women must disappear from Hobbes's account of the social contract and become subordinate to men. The social contract and absolute authority of the sovereign represent the turn to the father in the safe passage he offers away from nature and the power of the mother to "nourish, or expose" the child (Hobbes 1968, 254; see also Flax 1983).

Hence, it is difficult to understand what Pateman can mean by saying that "the classic contract theorists are hardly guilty" of saying that women are less than human, something she reserves for "extreme misogynists and patriarchalists" (p. 182); for if women's powerlessness is a necessary prescription for men's activity in politics, and public and political activity are defined as the essentially human, then are women not prohibited from engaging in the activities that make them human? Are they not so prohibited on the basis that they are not, in fact, human? Women's humanity could not be completely obliterated, of course; if men are to attain recognition, women must retain enough humanity to give this recognition at least a minimal value.[9] But this is tacit at best; they are certainly less than *fully* human. Again, there is a discordance between the overt, conscious statements of liberal obligation theorists and the meaning that underlies those statements; while liberalism tacitly utilizes a notion of different degrees of humanity, it explicitly employs an either/or typology: humans participate, women do not.

Rousseau is only the most explicit example of this phenomenon. There is a superficial way in which Rousseau seems to recognize the humanity of women: Sophie's role in the home is claimed to be as essential to the successful working of the social contract as Emile's participation in the assembly. But the difference in their roles also reveals that Sophie is unilaterally Emile's inferior from a moral—not to mention a political—perspective, primarily because of women's animal sexuality (Rousseau 1979). Rousseau clearly saw that the domestic sphere was important; but in the process of its dichotomization from the public, women *are* dehumanized. Rousseau's reassurance that women are "separate but equal" belies the reality that draws him to this construction of public and private.

This logic underlies other social contract theorists as well, and accounts for the continued disempowerment and nonrecognition of women (see Hirschmann 1992, chaps. 1–2). Hobbes postulates sexual equality in the state of nature but then introduces a radical disjuncture in the effort to disempower women in the social contract. Similarly, Locke, while making certain gestures toward sexual equality (Sinopoli and Hirschmann 1991, 228), nevertheless requires women's obedience to husbands to make his theory work. In defining the public as the realm of the essentially human and the private as tacitly important but overtly unimportant, women are by definition less than human because they are less than men.

The issue of sexual equality is directly tied to the priority of freedom; for as long as freedom is prior to other sorts of concepts, women will continue to be considered men's inferiors. Politically, the centrality of freedom allows men to continue to shun their nonconsensual obligations in the name of freedom. It leaves intact a public ideology that somehow, women are less human than men because they cannot protect their freedom by asserting themselves, standing up for themselves, by breaking free of their ties. Instead, as Gilligan's studies argue, women's experience suggests a different reality (Gilligan 1982; Gilligan, Lyons, and Hanmer 1990). It can reveal a moral schema that gives priority to aspects of human life that liberalism either makes invisible or allows the powerful to repress when convenient.

Hence, I agree with Pateman that liberal theorists "have to admit women's freedom at the same time that they deny it" (p. 000). Our difference seems to be that she wants to focus on the admission, while I seek to understand the denial. I take my approach because in understanding the denial, we unearth the theories' contradictions so that we may attempt to resolve them through a new theory. Certainly, "a new story about freedom is urgently needed" (Pateman 1988, 232–33); and feminists should be engaged in its telling (Hirschmann 1990). But freedom, from a feminist standpoint, must undergo a radical shift; it must be reconceptualized as part of other concepts, such as obligation, equality, and justice. It entails the priority of connection and relationship, enabling us to see that the well-being of others may enhance one's own capacity to act. In the process of this telling, it becomes clear that freedom is decentered: theoretically, it cannot sustain the hegemonic centrality that it occupies in liberal theory and simultaneously work

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interactively with these other concepts. This does not mean that freedom is not vital or should be jetisoned. Indeed, I clearly reject such a view (Hirschmann 1989, 1242; Sinopoli and Hirschmann 1991, 229–30). Rather, to paraphrase Pateman's closing sentence, the question is whether a political order in which all are *truly* free can be created if care is relegated to the shadowy corners of a devalued and segregated private sphere.

EPISTEMOLOGY

While I have argued that Pateman's earlier work extends liberalism even as it claims to challenge it, I have read her later work as agreeing with my arguments. In *The Sexual Contract*, she holds that "in modern civil society individual freedom is unconstrained" for men only because of the "mastery and obedience" of women and that accordingly, "[a] free order cannot be a contractual order" (1988, 232). She problematizes the meaning of *individual* and links this to patriarchal dualities: "When feminists forget that, though acceptance of the 'individual' may be politically necessary, so also is rejection, they acquiesce in the patriarchal construction of womanhood" (p. 14). And, of course, she has just mentioned "social individuality." In such passages, she seems to be working toward the transcendance of duality and toward the feminist perspective of connection and relationship that I develop through my feminist analysis of obligation.

But these apparent similarities mask a deeper division between us which Pateman may or may not recognize but certainly does not discuss, a division over the epistemological issues that lie at the heart of my argument. Neither Pateman nor Sinopoli have acknowledged my key claim that structural gender bias is structural because it is epistemological. This certainly resulted in Sinopoli's missing the point of my article, and I believe it affects Pateman's reading as well. Pateman holds that the approach of object relations is unnecessary because "the case can be made that standard arguments about political obligation are patriarchal from a consideration of classic and contemporary arguments" (p. 179).[10] By this, I assume she means that political obligation theory holds internal contradictions that do not make sense of women's exclusion from political power. I agree that such a case can be made. She herself makes that case admirably; and I engage in a similar analysis (Hirschmann 1992, chaps. 1–2). But that hardly precludes the epistemological argument. Indeed, it emphasizes the need for it, because it highlights the question, Why is it that such contradictions have been historically tolerated in theorists' efforts to exclude women from political power? How and why has liberalism sustained itself in the face of these contradictions?

Pateman says that "the classic contract theorists had reason to attack the divine right of kings. . . . they had no wish, however, to overthrow men's jurisdiction over women . . . because they had vital interests at stake *as men*" (p. 181). She thus suggests a conspiracy: men simply have not wanted to give up power. While this may be true in many ways, it does not go far enough; or perhaps it goes too far in attributing such motives to men. As I have emphasized in my two earlier accounts, men suffer from the reified and abstract presentation of obligation almost as much as women, though their position of power has allowed them to circumvent many of its disadvantages. My approach, in contrast, shows why men might be unwilling to give up such power; why such unwillingness might exist without consciousness of that power (or indeed, of that unwillingness); and thus also why it might endure even in the face of feminist arguments showing the hypocrasy and self-contradiction of liberalism. My approach also indicates how such power can translate into particular constructions of the political and how social relations can be structured to support that vision of politics.

Attention to such epistemological issues is what gets feminists beyond "Men are the problem, feminism the solution." By understanding the sexism of liberal epistemology, we can see that both men and women are caught in a particular construction of reality that is simultaneously a product and a constrictor of human action. If the entrenchment in liberal theory of the public/private dichotomy guarantees the obliteration of women's experience from our ordinary-language understanding of political obligation, and if gender bias relates to the very epistemological framework that defines the conceptual terminology in which theories operate, then any analysis of "the problem of women" within the boundaries of existing theories of obligation will only be able to go so far. Such analyses will have only limited depth by definition, because our conceptual vocabulary is itself biased.

A consideration of epistemology helps clarify the connections between choice, relationship, and obligation. Whatever women's history of coercion under patriarchy has been, contemporary women do have more choices than ever before. These choices present perplexing and often intractable dilemmas, but they mark women's ability to assume obligations as they see fit and to act as free agents. Indeed, these choices are in part due to liberalism—not the natural evolution of liberal theory itself but *feminist* efforts to push the logic of liberalism to its extreme conclusion (Hirschmann 1989, 1240).

Yet even if we say that "women" have more choices, the question as to *which* women really have those choices and what those choices *are* remains crucial. The feminist literature that informs my own argument suggests that to the degree that such choice is genuine, women's experience reveals that choice, though important, is not enough. It would seem that the original situation imposed on women by their nonconsensual obligations reflects particular aspects of *human* existence that should not be simply abandoned as relics of a patriarchal past but explored and examined as material for new feminist understandings of human relationship. Such examination is

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necessary to change the patriarchal structures within which women are fated never to have as much choice as men.

This might seem circular; but it is so only from the standpoint of liberalism's dedication to the dichotomy between free men and subservient women. Taking obligation as a *starting point* is very different from taking it as an *end point*; and this is the significance of taking given obligation as an epistemological and methodological claim (see Hirschmann 1989, 1243). Treating given obligation as an end point results in the preservation of the oppressive status quo that Pateman rightly fears. But as a starting point, from the perspective of women's experience, nonconsensual aspects of human relationships are vital to our humanity. Rather than segregate them in the private sphere and consign them to nonpolitical female nonactors, feminism requires that men, as well as women, recognize and fulfill these given obligations. Only then can both women and men take part in greater and more genuine choice.

In the process, of course, the dichotomy between choice (freedom) and choicelessness (bondage) is transformed into a relationship between the two, since it is revealed that many aspects of choice are contextual and intimately tied up with things about which we have no choice. Feminists cannot end the dualities of "natural/civil, private/public, women/individual" that Pateman rightly locates at the heart of the patriarchy of liberal theory (1988, 232) without also challenging dualities between voluntaristic obligations and "natural" duties, choice and choicelessness, and freedom and obligation. By failing to recognize that this requires an alternate epistemology and by failing to note how this *must* include some idea of nonconsensual obligation, her solution tells only half the story—and it is uncomfortably like the patriarchal half. Indeed, many of her remarks here read as if she seeks only to fine-tune, rather than transcend, liberal paradigms.

Understanding the epistemological focus of my work helps reveal why consent theory is so problematic for feminists and how gendered experiences help produce particular perspectives that explain the representation of reality that we find in liberal theory. One can never reduce anything as complicated as patriarchy to a single cause, as I have indicated (Sinopoli and Hirschmann 1991, 226); but that does not mean gender psychology and standpoint epistemology are not useful in explaining and understanding the duration of patriarchal aspects of political thought. This concern with explanation does not make my argument reductively causal any more than it ignores questions of cause. I addressed the question of causation versus meaning in my reply to Sinopoli, and Pateman is not clear why my remarks do not satisfy her. However, Pateman's charge that I "draw back from causal argument" (p. 180) to bring in the issue of interpretation is severely misleading: How can I "draw back" from a position I never took in the first place? Indeed, I was critiquing precisely the sort of reductive and dichotomous treatment of cause that Pateman seems to endorse.

Feminism is certainly interested in challenging such dualistic approaches to cause and meaning (Harding 1986). Perhaps the problem is due to a failure to distinguish between reasons and causes. *Reasons* might better capture the multifacetedness of a feminist vision of explanation; for it allows for the synergistic, dynamic, and interactive qualities of human relationships without either abandoning the question Why does this happen? or enshrining it in some social-scientific charade. Or if the language of causation must be used, perhaps it should be considered a *contributing*, rather than an *efficient*, cause; if the *final* cause of a political theory lies in particular historical events, such as the exclusion crisis or civil war, the social relations of childrearing create the psychological and cultural environment that influences how theorists choose to respond to these political phenomena. To ignore such cultural aspects seems as misguided as to reduce childrearing relations to a simple, singular cause of everything we find in liberal theory.

Feminists do not have to endorse the exact same approaches in order to be effective; nor can we assume that any individual author has the final word on a given topic in feminist or political theory. I think it is important for nonfeminists in political science to understand this about feminism. So, it is particularly gratifying that two feminist political theorists are able to engage in such a debate within these pages. Certainly, Pateman and I can agree that the differences between us are less important than our common enterprise of exploring how the experiences of women can help us reformulate the way we conduct political theoretical inquiry.

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Notes

Hirschmann thanks Julie Mostov and Ian Shapiro for their comments and suggestions.

1. Rather oddly, Hirschmann gives an abstractly individualist account of promising. She writes, "The purpose of promising as an institution is to ensure that people will keep their word" (1989, 1239). But the practice of promising *is* institutionalized word keeping (can anyone except an abstract individualist ask about its purpose?). The attempt to ensure that people keep faith is made through a variety of means.

2. Sinopoli appears to present a similar objection (Sinopoli and Hirschmann 1991, 223); but he refers to preliberal theorists and so completely misses the point that general arguments about political life that rely on abstract individualism are specifically modern doctrines.

3. Hirschmann neglects these aspects of consent, which are related to the "obligation" that women are held to have to allow men sexual access to their bodies. It can also be noted that some women now become pregnant through artificial insemination to avoid relations with men, because of fertility problems, or to become "surrogate mothers." Another set of questions about consent and power are raised by these issues.

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4. In an argument calling for an end to dualisms, her account of freedom depends on a very blunt dichotomy between negative and positive freedom. Hirschmann dismisses Rousseau from consideration on the grounds that since he has a "positive" view of liberty, he is not a liberal (1989, 1233), but it is not only liberals who are concerned with political obligation.

5. Hobbes was reared in a household full of women; but Hobbes is the only classic contract theorist (and one of the few political theorists) to start from the premise that women by nature were as free as, and equal to, men. This seems to cast doubt on Hirschmann's claims about mother-only childrearing.

6. Pateman (1979) insists on a distinction between consent theory and her own theory primarily because in the former we have no influence over the content of what we may or may not consent to, while in the latter such influence is paramount. But surely, this is only a distinction between "liberal democratic" consent and a more radical, egalitarian consent. Consent can be a matter of meaningful choice (e.g., my husband's and my negotiating a schedule for sharing child care), as well as empty and meaningless (an illegal alien's accepting below-subsistence wages). And while the former is clearly superior to the latter (which is exemplified by social contract theory), it nevertheless operates from similar assumptions about individuals. While I certainly support the goal of enlarging women's choices, the feminist literature I draw on suggests that social relationships will always contain some elements of nonconsensual obligations. As long as these are denied, the abstraction of individuals from social relationships persists. See Hirschmann 1992, chap. 2 for a fuller discussion of my views on Pateman's theory of obligation; see Mapel 1990 for a similar (though not feminist) critique of Pateman.

7. Similarly, the "caring" of noblesse oblige to which Pateman refers in closing hardly fits the account of caring provided by Gilligan (1982; see also Gilligan, Lyons, and Hanmer 1990), which requires mutuality and interaction among subjects, rather than the imposition of the privileged person's perspective of how care is to be provided for others.

8. Indeed, one could argue that in fact this is precisely what Locke does, particularly in *Questions Concerning the Law of Nature*, where he distinguishes between "reason" and "right reason." Locke might hold that God purposely makes some men irrationally "love the darkness" precisely so that all men will inevitably move to the social contract; this would then support Locke's implicit claim that his social contract is the only one that fits the divine order. But this even more strongly raises the question of why he felt compelled to hide this behind such a disingenuous account of consent. See Locke 1990, esp. quest. 1; cf. Hirschmann 1992, chap. 1.

9. While the Hegelian master/slave dialectic from which I develop the theme of recognition (Hirschmann 1989, 1236–37) indicates that the master (men) is dependent on at least a minimal human consciousness on the part of the slave (women) for recognition, this could in fact be carried further. For men *could* receive recognition from animals and objects, just as Hegel's slave receives it from nature. Women have certainly been considered animals, objects, and closer to nature by political theorists—thus negating their humanity altogether, not merely diminishing it.

10. This is a particularly curious claim, since Pateman herself uses Freudian theory as an interpretive tool for reading the sexual contract into liberal theory. Furthermore, I agree with Okin (1990, 664) that Pateman misreads Chodorow (1978), whose work would be more useful than Freud's to Pateman's argument.

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NOTES FROM THE MANAGING EDITOR

New Size and Familiar Contents for the APSR

You may have noticed that your March, 1992, *APSR* will no longer align comfortably on the shelf with its predecessors. Despite the disadvantages of discontinuity, the reasons for the change are compelling. The changes were initially proposed by the APSA staff and supported by the Publications Committee of the Association. The new 8 1/2" by 11" size saves substantially on paper costs, as trimming and wastage are no longer necessary. The savings will be used in part to contain general costs for the Association and in part to support an expansion of the *Review* by about 10%. We need the additional size to keep our article acceptance rate in the one-of-ten range and to accommodate the needs of the *Book Review*. (In the current issue, the additional space is being used for backlogged book reviews.) Moreover, the larger page size will make it easier to print readable figures, tables, and equations. Our shift to a new press should also enhance our technical capability. The larger size has required a few design changes, but for the most part we have tried to stay close to the excellent format developed by Pat Patterson and earlier managing editors.

Of course, the essence of the *APSR* is not its physical appearance, but its articles, essays, and reviews. In this regard the reader will find continuity rather than change. It is part of the mission of the *APSR* to be a major vehicle for outstanding scholarly research in political science in all of our subfields. I hope that perhaps we can also help those subfields to communicate with each other.

The determination of quality is made primarily through a peer review process. Almost all of the submitted manuscripts (currently pouring across our desks at a rate of about 50 a month) are assigned referees familiar with the manuscript's subfield. In each panel of reviewers we try to have at least one technical expert and at least one reviewer who can address the importance of the research from a broader perspective.

The selection of appropriate reviewers is obviously the key to selecting the best manuscripts. It is also essential to what Pat Patterson has called "the seminar by mail"—providing rejected manuscripts with high quality critiques and advice. We review manuscripts that not only focus on different subjects in the world of politics but draw on quite different paradigms of theory and method. It is difficult for any one scholar to have mastery of more than a fraction of our large and diverse discipline. I have tried to respond to the challenge of such diversity by checking our intuition about reviewers for each manuscript with at least one member of our new (and hard-working) Editorial Board.

The most important task of the Managing Editor of the *APSR* is to maintain the scholarly excellence of its contents. I also hope to recapture the remarkably rapid and efficient processing of manuscripts that Pat Patterson developed in his six years as Managing Editor. Even with the aid of his procedures manual, a hard-working staff and a willing Editorial Board, this is proving a formidable task. But whatever the stresses of adaptation, I trust that the *APSR* shall demonstrate that an increase in size does not imply a decrease in quality.

G. Bingham Powell, Jr.

FORTHCOMING IN JUNE, 1992

The following articles, research notes, and controversies have been tentatively scheduled for publication in the June, 1992 issue:

Jonathan Bendor and Thomas H. Hammond. "Rethinking Allison's Models."

Jeffrey Berejikian. "Revolutionary Collective Action and the Agent-Structure Problem."

Steven Forde. "Benjamin Franklin's *Autobiography* and the Education of America."

Tracey E. George and Lee Epstein. "On the Nature of Supreme Court Decision Making."

James L. Gibson. "The Political Consequences of Intolerance: Cultural Conformity and Political Freedom."

Arthur W. Lupia. "Agenda Control Versus the Power of Information."

Elinor Ostrom, James Walker, and Roy Gardner. "Covenants With and Without a Sword: Self-Enforcement Is Possible."

Yoav Peled. "Ethnic Democracy and the Legal Construction of Citizenship: Arab Citizens of the Jewish State."

Benjamin Radcliff. "The Welfare State, Turnout, and the Economy: A Comparative Analysis."

Laura L. Stoker. "Interests and Ethics in Politics."

Viktor Vanberg and Roger D. Congleton. "Rationality, Morality and Exit."

Robert Bates and Amy Farmer Curry. "Community Versus Market: A Note on Corporate Villages." A research note.

Reginald S. Sheehan, William Mischler and Donald R. Songer. "Ideology, Status and the Differential Success of Direct Parties before the Supreme Court." A research note.

Robert S. Erikson, Michael B. MacKuen, and James S. Stimson; Paul Abramson, and Charles Ostrom. "Macropartisanship." A controversy.

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BOOK REVIEW ESSAY

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COLLECTIVE WISDOM

JOHN C. WAHLKE *University of Arizona*

The Analysis of Political Structure. By David Easton. New York: Routledge, Chapman, and Hall, 1990. 336p. $37.50.

The Rhetoric of Reaction: Perversity, Futility, Jeopardy. By Albert O. Hirschman. Cambridge, MA: Harvard University Press, 1991. 197p. $17.95.

Inquiry and Change: The Troubled Attempt To Understand and Shape Society. By Charles E. Lindblom. New Haven: Yale University Press/New York: Russell Sage Foundation, 1990. 314p. $29.95.

American Citizenship: The Quest for Inclusion. By Judith N. Shklar. Cambridge: Harvard University Press, 1991. 120p. $17.95.

The Faces of Injustice. By Judith N. Shklar. New Haven: Yale University Press, 1990. 144p. $19.95.

Models of My Life. By Herbert A. Simon. New York: Basic Books, 1991. 415p. $26.95.

The authors of these diverse works are eminent in their own fields; but they are also senior professionals in the social science community, with experience in numerous government, university, and organizational capacities (including, in several cases, presidencies and other high offices), as well as in research and teaching. They write from a perspective enjoyed by few of their colleagues.

Recent years have witnessed increasing signs of malaise in and about the social sciences. The fact that all the authors were cognizant of, and concerned about—and some participated in—the disaffections of the 1960s and various earlier and later controversies suggested my question: Besides what each book tells us about its particular subject, what might we also learn—whatever our discipline or field of special interest—by viewing them all together in that light?

BRIEF SUMMARIES

Easton says that *The Analysis of Political Structure* is "a book about general theory" (p. ix); it is not itself a theory. It provides "identification, justification, and substantiation of the problem" (p. 282); that is, it tells us how to think theoretically about political structures. These are not visible as physical things, but are "phenomena [that] ought to be considered only as *properties of things*" (p. 30; emphasis original). Yet they are empirically identifiable phenomena and therefore measurable (in the generic sense). In my own words, structures exist in the *relationship* among things: it is the relationships that are empirically observable as properties of the behavior of people. What makes structure politically important is that it constitutes a *constraint* on people, "a limit on the variety of choices open to an individual or collectively" (p. 52).

Easton makes important distinctions (1) among *formal structures* (created by conscious human action, commonly described in constitutions and formal documents and *crescive* structures (developed over time without conscious intent); (2) between *lower-order* and *higher-order* (global) *structures*—directly observable in behavior and not directly observable (but also not abstract or metaphysical) respectively; and (3)—most crucial in Easton's conception—between *lower-level* and *higher-level* structures. His example of a higher-level structure is the political regime, which, although superficially of the same level as its four main explanatory variables (biological organism, political personality, political culture, and political behavior, each of which is also a structure), is, in his analysis, higher-level. He amplifies these distinctions and such related key concepts as institution, group, party, and so on, by differentiating "structuralism as a theory" from "structural theories."

Part 2 analyzes the work of the post–World War II structuralist Marxist, Nicos Poulantzas. Easton gives him credit mainly for being on the right track, in that "by implication, he addressed political life as a system of some sort" (p. 237) and thereby lent support to Easton's major point, as stated in the prologue: "If we are to understand the shape taken by regimes, we shall be forced to see them in the context of the structure of political systems viewed as wholes" (p. xiv). Following further conceptualization of higher-order structures and other concepts in his scheme, Easton explains the framework of his thinking, declaring himself to be a "pragmatic holist" but a "*theoretical* methodological individualist" (pp. 256–57; emphasis Easton's). In the concluding chapter he restates what his contribution has been and explains why, as he forewarned, he has not produced a theory of political structure:

> Our identification, justification, and substantiation of the problem provide the necessary if not inescapable first step. . . . It is one thing to recognize the possible existence of higher-order structures and their likely influence on lower-order ones and, thereby, on the disposition of political power in a political system and on control over outputs (public policies). It is quite another thing to identify a research technology that will permit us not only to describe this higher-order structure but to trace out the ways in which it facilitates, determines, or limits lower-order ones. (p. 282, 285)

That is to say, theorizing has outstripped research technology. The implications of that (if true) will be discussed, as will a related problem that might also interfere with the further development of theory, namely, the plight of the general theorist in perilous times, described by Easton in the final two pages of his book.

Hirschman's *Rhetoric of Reaction* analyzes the rhetoric used by reactionaries in political controversies in

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three different periods spanning several centuries and representing three stages of the development of political rights: (1) civil liberties, (2) political rights, and (3) socioeconomic rights.[1] The authors whose works constitute his data concerning the first period, which culminates in the French Revolution and the Declaration of the Rights of Man and its echoes elsewhere, include Edmund Burke, Joseph de Maistre, Adam Muller, and Alexis de Tocqueville. Those exemplifying the second epoch, characterized by the rise of universal male suffrage and democracy, include George Canning, Robert Lowe, Sir Henry Maine, Fustel deCoulanges, Max Scheler, Gustave leBon, Herbert Spencer, Gaetano Mosca, Vilfredo Pareto, and James Fitzjames Stephen. For the third period, characterized by the rise of the welfare state, he cites Friedrich Hayek, Samuel Huntington, Opponents of the English Poor Laws and advocates of new Poor Laws, Jay W. Forrester, Nathan Glazer, Charles Murray, George Stigler, Martin Feldstein, and Gordon Tullock. By subjective content analysis of a nonrandom sample of polemical participants and commentators who reacted against these developments, he offers "a 'cool' examination of surface phenomena: discourse, arguments, rhetoric, historically and analytically considered" (p. x). Hirschman finds "a few common or typical arguments unfailingly made by each of the three reactive movements" (p. 7), which he describes and illustrates in a chapter each.

The *perversity thesis* "is, at first blush, a daring intellectual maneuver. The structure of the argument is admirably simple, whereas the claim being made is rather extreme. It is not just asserted that a movement or a policy will fall short of its goal or will occasion unexpected costs or negative side effects; rather, so goes the argument, *the attempt to push society in a certain direction will result in its moving all right, but in the opposite direction*" (p. 11; emphasis original). Hirschman believes that this argument "is unlikely to exist 'out there' to anything like the extent that is claimed" (p. 35). Examples of the use of this argument are offered from the writings of Burke, deMaistre, and Adam Muller for the first period, leBon and Herbert Spencer in the second, and Forrester, Blazer, Murray, along with polemecists in the Poor Law controversies for the second, and Stigler, Feldstein, and Tullock for the third.

The *futility thesis* argues "that the attempt at change is abortive, that in one way or another any alleged change is, was, or will be largely surface, facade, cosmetic, hence illusory, as the 'deep' structures of society remain wholly untouched" (p. 43). This argument amounts "to a *denial or downplaying of change* in the face of seemingly enormous epochal movements" (p. 70; emphasis original): "In its scenario, human actions or intentions are frustrated, not because they unleash a series of side effects, but because they pretend to change the unchangeable, because they ignore the basic structures of society" (p. 72). Among users of the futility thesis Hirschman includes de Tocqueville (first period), Mosca, Pareto, and Stephens (second period), and Stigler, Feldstein, and Tullock (third period).

The *jeopardy thesis* is "a more commonsensical and moderate way of arguing against a change which, because of the prevailing state of public opinion, one does not care to attack head-on: . . . it asserts that the proposed change, though perhaps desirable in itself, involves unacceptable costs or consequences of one sort or another" (p. 81). Users of this argument include Canning, Lowe, Maine, deCoulanges, and Scheler (in the second period), and Hayek and Huntington (in the third).

These three themes, Hirschman says, "account for the bulk of the rhetorical assaults I have undertaken to analyze" (p. 136). Logically, they reduce to two basic reasons for opposing a reform proposal: (1) it did not accomplish its purpose (perversity and futility) or (2) the costs incurred outweigh the benefits (jeopardy). Believing that 'Reactionaries' have no monopoly on simplistic, preemptory, and intransigent rhetoric [and that] their 'progressive' counterparts are likely to do just as well in this regard" (p. 149), he then turns the tables and shows that liberals and progressives use mirror images of the reactionary thesis. Instead of the incompatibility of reforms with other ends—for example, liberty versus democracy (jeopardy thesis)—they argue the case for their mutually supporting one another (the synergy illusion).

Similarly, where the reactionaries' jeopardy thesis posits threats posed by a proposed reform, liberals and progressives justify proposed reforms with an "imminent danger thesis," citing the dangers posed by *in*action, for example, threats to democracy from citizens made increasingly desperate by an inequitable welfare system. And where reactionaries argue the futility of reform action taken in the face of laws of social inertia, liberals and progressives discover laws of historical movement and progress (the thesis of "having history on one's side").

Hirschman aims at more than merely mapping "the *'rhetorics of intransigence'* as long practiced by both reactionaries and progressives" (p. 168; emphasis original). He wants to point to their survival in contemporary ideological discourse about domestic government—about which he is not overly optimistic, given its origins in ideological controversy. "Tolerance and acceptance of pluralism," he argues, "resulted eventually from a *standoff* between bitterly hostile opposing groups" (ibid.; emphasis original)—hence his "cool" method of examination, chosen to help "move public discourse beyond extreme, intransigent postures of either kind, with the hope that in the process our debates will become more 'democracy friendly' " (p. 168).

Lindblom's *Inquiry and Change*, rather than being about how people talk, is about how they think in politics, specifically "how people in contemporary industrialized societies . . . go about gathering and analyzing information in grappling with social problems. By people," he emphasizes, "I mean all kinds" (p. vii).

His concern is with problemsolving, but he thinks

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terms like *attacking* or *grappling with* are more appropriate, since the process is one of unending inquiry. The motive force of this process is *Volitions* which arise from a person's sense that something in society is wrong or lacking and that something ought to be done about it. From this, "building on a foundation of fact, poorly or well constructed, people (never alone but always in social interaction) form, create, decide, will. . . . On how to live—what wants and interests to pursue—at every point and for every facet of this question, people create rather than find. Call what they create a volition. It might be called a judgment, but 'volition' has the advantage of stressing commitment or will" (p. 21).

Probing volitions is the crucial step, or element, in attacking problems effectively. The term emphasizes that the concern is for "thinking that is engaged in real-world problem solving" (p. 35), not just formal policy thinking. Probing is a continual, complex, and unsystematic process. It includes trying to define problems, thinking about normative and evaluative questions, wondering about facts, and logically organized inquiring and dreamy rambling: "Everybody always probes. However badly, all of us engage in examining volitions, even while asleep and both consciously and unconsciously in waking hours. . . . Inquiry mixes with and advances with action" (p. 30). The structure of their probing "takes the form of a web rather than a hierarchy" (p. 40) of means–ends overarching values. Means–ends analysis therefore becomes "a simplifying cogitative strategy" (p. 41), or heuristic, an echo of Herbert Simon and contemporary proponents of information processing.

Lindblom analyzes an important phenomenon that he calls "impaired inquiry" at the macro level and "defective thinking about social problems" at the micro level. He emphasizes that "we are all imperfect or defective investigators" (p. 61). We have to live with the fact that "biology limits the human capacity to probe. It also programs everyone to indulge in various 'irrationalities' for various reasons" (p. 61). Other agencies of impairment are not wholly beyond change or control, however. The two main categories of defects he examines are those which are socially caused (by, e.g., inadequate competition of ideas or socialization) and those arising from the advantaged positions of various participants in the process of probing, including the positions of parents over children, teachers over pupils, foremen over workers, whites over blacks, as well as elite positions in the communications media, education, business, religion, and government.

What can social science do about this? Lindblom's answer is straightforward and unflattering. He "cannot identify a single social science finding or idea that is undeniably indispensable to any social task or effort" (pp. 136–37). He questions applications of natural science models of science to political and social affairs, whose "mentalistic" phenomena are not amenable to explanation and prediction. But he does admit that social scientists can perform other useful intellectual tasks, including reporting (probably the main contribution social science has made and might make), evaluation (although "some heroic attempts at escape from evaluation continue in social science" [p. 147]), concept analysis and cognitive structuring (necessary for any inquiry into the world), checklists and scenarios, "meaning," synthesis and design, and methodology.

In contrast to a "science-guided society," Lindblom depicts a "self-guiding society," characterized by attacking problems with intelligent probing, freed as much as possible from impairments of inquiry. But probing here (unlike the more narrow probing of science) does not assume that with sufficient analysis every problem has a correct solution or pretend ability to determine the correctness of feelings or volitions. Social science here supplements, rather than replaces, lay probing. And it is much less hostile to power than are some versions of the science and other models—recognizing that "probing, discussion, and persuasion cannot take a society all the way to solutions" (p. 222). It requires, of course, a democratic, pluralistic framework within which to act.

Shklar argues in *American Citizenship* that U.S. citizenship has always been a matter of standing, rather than power, "an affirmation of belonging rather than the exercise of a right" (p. 26): "The tension between an acknowledged ideology of equal political rights and a deep and common desire to exclude large groups of human beings from citizenship has marked every stage in the history of American democracy. And it was this juxtaposition of slavery and constitutional democracy, above all else, that set America apart from other modern states" (pp. 28–29).

She demonstrates this by analyzing development of the right to vote and the right to gainful employment, contrasting citizenship-as-standing with conceptions of citizenship that played lesser, if any, roles in the United States: citizenship as participation alternately in ruling and being ruled (Aristotle), the citizen–soldier (Machiavelli), the loyal, obedient subject (Bodin and Hobbes), the citizen–proprietor (Turgot and others).

The case with respect to voting rights is documented by examining actions and behavior, as well as arguments, of proponents and opponents of extensions of the suffrage. A powerful bit of evidence, for example, is the failure of more than half the beneficiaries to exercise the vote after it was won (substantially more, in the case of youths, who were given the right without even asking for it). The evidence shows that the claim for extension of the rights of citizenship was in each case motivated by concern of the excluded for standing: "Not the exercise, only the right, signified deeply" (p. 27). More important, the fear of exclusion was expressed as a fear that without the vote as a symbol of citizenship, they would be no better than slaves.

After the erosion of property qualifications and enactment of the Fifteenth Amendment, race and gender replaced property as qualifications: "When disfranchised women saw black men achieve a right

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that they still lacked, their deep racism quickly asserted itself, and it grew worse as they began to seek the support of Southern women. . . . This unhappy chapter in the women's suffrage movement . . . illuminates the darker side of citizenship as standing" (p. 57).

The right to vote has been extended to virtually all beyond a minimum age. But the desperate need for standing as a citizen now finds expression as a claim to the right of gainful remuneration: "The individual American citizen is in fact a member of two interlocking public orders, one egalitarian, the other entirely unequal. To be a recognized and active citizen at all he must . . . also be independent, which has all along meant that he must be an 'earner,' a fee remunerated worker, one who is rewarded for the actual work he has done, neither more nor less" (pp. 63–64).

Shklar finds that here, too, the efforts of workers from revolutionary days down to the present have been to achieve secure status as full members of the community (citizens), not just economic security or monetary gain. Today the Jacksonian ideology "has attached itself not to the right of the worker who demands respect in addition to an honest day's wage for an honest day's work, but to the unemployed. They want to work so that they can reclaim their citizenship as well as their paycheck. Who wants to be 'nothing'?" (p. 95). She finds the same logic motivating welfare recipients today.

Shklar's book is one for all U.S. citizens, as well as a scholarly interpretation to be profitably read by historians and political theorists; for voting and earning, "as they have emerged out of the stress of inherited inequalities, especially the remnants of black chattel slavery, in a society committed to political equality and to the principle of inclusion," are "the most elementary and essential components" (p. 101) of U.S. citizenship.

In *The Faces of Injustice*, instead of the customary question, What is justice? Shklar asks the deceptively simple question, "When is a disaster a misfortune and when is it an injustice?" (p. 1). Putting it this way requires looking at it from the victim's perspective. In any misfortune, the human tendency is for bystanders to see the injured party as a victim of accident or disaster, while victims instead feel aggrieved by an act of injustice, even in the case of an obvious "act of God." When victims' sense of injustice is aroused, they expect redress through the institutions of "primary" justice (Shklar's "more neutral" term for distributive or procedural justice), which assigns status and entitlement of members of the polity by legal rules; or they may perhaps seek revenge or retribution elsewhere. But those institutions, Shklar says, are blind to the possibility of *"passive injustice"*—meaning not "our habitual indifference to the misery of others, but a far more limited and specifically civic failure to stop private and public acts of injustice" (p. 6). And this makes answering her opening question much more complex and difficult. Involved individuals' drawing of the line between injustice and misfortune is subjective, and how they draw it in particular cases is an empirical question. But how polities draw the line is a political question, not a matter of prior legal definition.

Passive injustices and the sense of injustice aroused by them are a critical problem for all government and societies; for even a few citizens outraged at injustice can be a threat to public order, and larger numbers of them a threat to the polity itself. At the same time, the sense of injustice—"the special kind of anger we feel when we are denied promised benefits and when we do not get what we believe to be our due" (p. 83)—is "our best protection against oppression" (p. 85): "In the absence of realistic opportunities for choice, voice, protest, and denial, [the rules of primary justice] are nothing but incentives to injustice (p. 116). Democratic thinking accepts expressions of injustice as an appropriate reaction to an unwarranted social deprivation" (p. 84). Constitutional governments tolerate the expression of citizens' felt sense of injustice.

In short, what citizens consider to be injustice is an empirical, not a normative, question. How they will express their felt sense of injustice is also an empirical question. What legal procedures ought to be open to them to do that, however, is a political, not a legal, question. Shklar's message hardly stops there. It has strong implications for public policy, public law, and civic action. "This whole book is really about America" (p. 6), she says. Like *American Citizenship*, it is a book for all U.S. citizens and not just academe.

Herbert Simon is a scholar unique in the breadth of his talents and skills, the range of his interests and corresponding range of his activities, and the level of his attainments. His comprehensive, candid, and honest autobiography, *Models of My Life*, will pleasurably reward and instruct any reader about the objects of Simon's interest, as well as about the man himself.

His professional career began in public administration. He taught at the Illinois Institute of Technology and then at Carnegie–Mellon University (then Carnegie Institute of Technology) until his recent retirement. He was a moving force in establishing the latter's Graduate School of Industrial Administration and in bringing its business school to recognized national leadership. His departmental affiliations, like his work, spanned economics, psychology, and computer science. He was a frequent participant in academic government and politics as a faculty member, department head, and dean. He resisted the temptation to be a university president when offered the opportunity, but regularly took active part in the selection of top administrators, as well as in other university-level decision making.

He had continuing affiliate or consulting relationships with the Rand Corporation and the Cowles Commission for Research in Economics and consulting relationships with the Ford Foundation and others. He was active in the Social Science Research Council. He turned down opportunities to become president of the American Political Science Association but was active in it in many other ways, includ-

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ing participation in the Young Turks "rebellion," which led to the replacement of the then-incumbent executive secretary.

He became a fellow of the American Econometric Society and a Nobel Prize winner in economics—although classical economists generally shunned him for his advocacy of the concept of "bounded rationality." He had a major hand in bringing the social sciences into the National Science Foundation and the National Academy of Sciences, of which he is a member. He was a member of the Committee on Sciences and Public Policy (and the expanded Committee on Science, Engineering, and Public Policy) and the President's Science Advisory Committee.

His doctoral dissertation on public administration (published as *Administrative Behavior*) is generally credited with having been one of the earliest and most decisive steps in carrying out the "behavioral revolution" called for by Charles Merriam at Chicago (when Simon was a student there). He went on to play pivotal roles in the fields of organization theory and theories of the firm, artificial intelligence, cognitive science, and information processing. Besides seminal contributions in all these fields, he has produced major works in economics, econometrics, and decisionmaking.

Models of My Life is not a recitation or chronology of these accomplishments but a lucid account of each major enterprise in his career, including its substance, the rationale behind it (in terms intelligible to outsiders), and its politics. At relevant points throughout are statements and explanations of his thoughts, ideas, feelings, and preferences about issues, ideas, and practices of major concern to social scientists.

EMERGENT THEMES

Collectively considered, these six works offer much food for thought about scientists' social and civic role. Sheldon Wolin has long argued that political theory "is primarily a civic and secondarily an academic activity."[2] Donald L. Langenberg has recently called attention to "a damaging disjuncture between the community of scientists and engineers and the society in which the community is embedded," which he attributes to "our failure to do our civic duty."[3] "Our universities," he says, "and our science and engineering community can and must become more centrally and fully engaged with the great societal issues represented by the slogans I have listed and many others."[4] Such engagement is important for all scientists as citizens, but especially important for social scientists, because "societal reality is, in the end, determined by societal perceptions through the political process, regardless of whether those perceptions are grounded in fact. And that, unfortunately, is what gives truth to [the] adage, 'Only in politics are facts negotiable'" (p. 361).

Engagement with great societal issues is strongly evidenced by at least five of our six works: Shklar's analysis of the cancer of race, which continues to gnaw at the vitals of U.S. society, and her account of injustice; Lindblom's examination of social scientists' probing of social problems (evidence of both his own engagement and his concern to promote that of social scientists and lay citizens); Hirschman's application of his analysis of the rhetoric of intransigence to contemporary political discourse; and Simon's account of his career as a demonstration of the varieties of forms and fields in which engagement is possible. Easton alone, of this group, might appear to avoid confronting social issues. Whether one sees this as an evasion of civic duty or a legitimate desire depends on one's views about the utility of Social Science Knowledge.

What is the role of social scientists qua scientists in society? To do research purely for knowledge's sake or to do research devoid of theoretical basis but aimed directly at social problems? Easton does believe that "a deeper and richer understanding of political life" is desperately needed to cope with society's complex global problems and that general theorists and pure scientists contribute to that. But he also believes that long-range scholarship—today especially—requires insulation from "the unfortunate siege mentality forced upon [it], where the problems for inquiry will be "the problems that theory dictates as requiring attention, not necessarily [those dictated] by the pressing and urgent issues of the day" (p. 287).

By itself, that answer begs the question. The usual academic argument for it is that abstract ideas and even whole theories that at first lie fallow or are rejected outright might turn out in the long run to be founts of endless practical wisdom. It is therefore wrong to ask pure scientists to engage only in medium-range or short-range projects whose relevance to contemporary issues is apparent but whose long-run benefits are no more certain than those of pure theorists.

With respect to the issue of pure versus applied science, the two viewpoints are not mutually exclusive in the overall scheme of things, of course. Easton implies a functional division of labor between pure theorists and others: "I have assumed . . . that there is still room for at least some few scholars who continue the pursuit of basic knowledge" (p. 287). Nowhere does he express objections to research more directly relevant than the theorist's to pressing social problems. His argument includes the necessity for bringing general theory down to earth. But the implication is that lower-level conceptualization of the problems and issues and operationalization of theory relevant to them is primarily the task of scientists other than the general theorists.

Both Lindblom and Simon argue against such a division of labor. Lindblom can find little difference between the investigatory activities performed by professionals and lay probers: "Professional inquiry consists for the most part of lay inquiry done better" (p. 161). Both professional and lay testing "finally rest on judgment rather than some conclusive technique" (p. 169). Perhaps more important, professional social

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scientists, both pure and applied, are prey to inquiry-impairing defects peculiar to their own disciplines, which Lindblom discusses at some length.

Moreover, Lindblom points out, "Academic, discipline-oriented, professional inquiry not focussed on policy" and "policy studies that culminate in recommendation" are not the only two choices open, as some practitioners in the "subprofession of 'policy analysis' might imagine" (p. 271). A superior alternative is for advisers to "choose specific analytical contributions to fit the needs and competencies of the functionary. On most of the client's decisions, such analysts would remain silent. On a small number they would selectively combine their competence in inquiry with that of their clients. On only a small number of the client's problems would analysts proceed with the do-it-all or reach-a-recommendation model that some policy analysis textbooks advise" (p. 272).

Simon's position, like Lindblom's, argues for the merging—not the separation—of the efforts of pure and applied scientists. During faculty discussions of the curriculum at Carnegie–Mellon he proposed at one point "that the university establish a core curriculum for the faculty, requiring every CMU faculty member to pass the comprehensive examinations on the common core of the program within the next four years. . . . Then [says Simon] came the punch line: a faculty that is not liberally educated can't provide liberal education. American college faculties, including our own, aren't liberally educated" (p. 267). Despite the rigor and "scientific" discipline of his own work, Simon is no believer in "the myth of the 'exact sciences' or 'hard sciences'. . . . The true line is not between 'hard' natural science and 'soft' social sciences, but between precise science limited to highly abstract and simple phenomena in the laboratory and inexact science and technology dealing with complex problems in the real world" (p. 304).

Similarly, with respect to the distinction between pure and applied science, Simon writes, "The distinction between the scientific and the professional is largely a distinction between analysis and synthesis. Professionals not only analyze (understand) situations, they act on them after finding appropriate strategies (synthesis)" (p. 258). Like Lindblom, Simon would bring applied and pure science practitioners together, not divide them functionally. Like Lindblom, Simon believes it is the more abstract, pure disciplinarians and academicians who most need persuading, as shown by his views on the unhealthy trend he saw developing in professional schools like engineering, business, and medicine, where "[pure] science was replacing professional skills in the curriculum. . . . Engineering schools were becoming schools of applied physics and mathematics, and medical schools were becoming schools of biochemistry and molecular biology. Professional skills were disappearing from the curricula and professionals possessing those skills were disappearing from the faculties" (pp. 257–58).

Collectively, our six works make an effective argument against the dangers of disciplinary parochialism, or balkanization, by avoiding it. They draw ideas and evidence from history, philosophy, and empirical studies in psychology and other fields—not just from political science. They are more "interdisciplinary" than disciplinary. Simon, however, besides spanning and applying his own four main specialties—and others as well—also speaks explicitly and eloquently about the problem: "Academicians typically live out their whole careers within the culture of a discipline, rarely shaking off the parochialism this isolated existence engenders" (p. 173). Again, disciplines, like nations are a necessary evil that enable human beings of bounded rationality to simplify their goals and reduce their choices to calculable limits" (p. 367). About area studies (which are often thought to be interdisciplinary), he observed, "Too often, they seemed to aim at training disciplinary specialization *within* area specialization: Experts of the Russian economy, the Chinese government, the Indonesian family" (p. 173).

It is easy to agree that a world in which "parochialism is everywhere . . . badly needs international and interdisciplinary travelers to carry new knowledge from one enclave to another" (p. 367), but it is not easy to know how to combat it. During the development of the Center for Advanced Study in the Behavioral Sciences, Simon advised the Ford Foundation that "the problem is less one of bringing unlike social scientists together than one of bringing unlike social sciences together in one man" (p. 170). But he "can offer one piece of advice to others who wish to try an itinerant experience: It is fatal to be regarded as a good economist by psychologists, and good psychologist by political scientists" (p. 367).

The lessons to be learned from reading these works (all splendid examples of "probing"), then, are more ethical instruction than empirical fact or theory. They help instruct us how to be good scholars and scholarly citizens not by maxim or catechism but by revealing the complexity of that goal and living up to it. They all agree that failure to extend our scholarly interests and conceptions outside our narrow disciplinary boundaries is bad scholarly performance. They make clear scholars' ethical obligation to be engaged citizens, not merely disengaged inhabitants, of the wider communities of disengaged inhabitants of which we are members. They also suggest that we have much more to learn.

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Notes

The credit for beginning this train of thought goes to Helen Ingram, who, as Book Review Editor, recognized almost at once upon receipt of them what the books had in common.

1. Hirschman's starting assumption was that in each of the three stages, and not merely the last of them, the initial thrust to extend the rights of citizenship was followed by "ideological counterthrusts of extraordinary force" (p. 3). T. H. Marshall, whose scheme Hirschman followed, hypothesized sturdy resistance to the initial movements for reform but eventual triumph for all three; Ralf Dahrendorf claimed the battle was never won in the third phase.

2. Sheldon Wolin, *The Presence of the Past* (Baltimore: Johns Hopkins University Press, 1989). 1. See also his *Politics and Vision* (Boston: Little, Brown, 1960) Harvard president Derek Bok has expressed doubts that universities and scholars are doing enough to develop and bring into play, in their own work and teaching, "a strong sense of civic responsibility, ethical awareness, and concern for the interests of others" (quoted by Donald L. Langenberg, 362) *Universities and the Future of America* (Durham: Duke University Press, 1990, 7).

3. "Science, Slogans, and Civic Duty," *Science Magazine*, 19 April 1991, pp. 361–63. Langenberg is chairman of the board of directors of the American Association for the Advancement of Science.

4. The applicability of the argument to social scientists is further indicated by the number of issues on Langenberg's list of 25 "slogans" (shorthand symbols symbolizing the issues) that are of obvious interest to them, including scientific freedom and integrity, "Set scientific priorities," "Save the environment," the information age, science literacy, educational reform, "Politically correct," multiculturalism, "No new taxes," and others.

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POLITICAL THEORY

Nazi Economics: Ideology, Theory, and Policy. By Avraham Barkai. New Haven: Yale University Press, 1990. 291p. $27.50.

As research and debate on German National Socialism have expanded during the past 30 years, opinion has sometimes divided between the so-called *intentionalists* and *structuralists*. The latter find little evidence of coherent ideology or grand design in the policies and government of the Third Reich, which they contend was severely conditioned by contingent circumstances, leading to one improvisation after another, though each in varying degrees constrained by what had gone before. The evidence they cite is at first glance extensive, consisting of the apparent contradictions in Nazi doctrines and Hitler's own statements, the absence of any single clear, unified ideological blueprint, the "organized chaos" of multiple conflicting and overlapping government and party boards and administrative units, and the apparent improvisation of so much of Hitler's timing and strategic gambles. In the most extreme formulations of this school, Hitler himself was a "weak dictator," not fully in charge of the German government and not necessarily responsible for some of its most extreme decisions.

The intentionalists reject such historiographic revisionism. Though they generally agree that National Socialism had no such formally developed and unified ideology as liberalism or Marxism–Leninism, they have concluded that Hitler had a drastic, clearly enough defined *weltanschauung* and, though always willing to compromise on details and short-term policies, held to his broader political and strategic goals with a grim ruthlessness equaled only by that of his grand rival and foe, Josef Stalin. He did not administer personally the Nazi state (for bureaucracy and microadministration bored him); but he guided it firmly in the direction that he sought, ultimately at the cost of risking total annihilation.

Avraham Barkai, a research fellow of the Institute for German History at Tel Aviv University, is a firm intentionalist. His study, which first appeared in a shorter version in German in 1977, does not deal with the broader dimensions of National Socialist ideology or administration, concentrating instead on the specific issues of economic doctrines and policy. In this field, too, the structuralists have defined the Nazi approach as an incoherent and opportunistic hodgepodge, lacking clear theory or system. Barkai provides the first systematic treatment of Nazi economic doctrines in the years prior to Hitler's takeover and sets it clearly in the tradition of the nineteenth-century "German economics" of statism and state socialism. He then goes on to examine in detail the policies and economic administration implemented by the regime during its first three years in power.

He convincingly concludes that though "Nazi ideology was not a unified and consistent conceptual system," the party "was unquestionably an ideological movement, even though its ideas were a jumble collected from here, there, and everywhere without any worthwhile original contributions" (p. 243). The product, by 1933, was a pragmatic collage that emphasized extreme state control and regulation, together with massive deficit spending and credit creation, aimed at ending unemployment and gearing the economy for war. Most general lines of economic policy had already been defined in statements and position papers before 1933. Though central planning was rejected, the Hitler state created a new framework of government control that reorganized and dominated the administrative units of industry and other major sectors, controlling prices and channeling profit, investment and raw materials. The market was not abolished, but it was firmly controlled. Unemployment was eliminated within four years, while production increased rapidly and inflation was effectively checked. The goal was autarchy and self-sufficiency—and rapid reconversion for military expansion. In this regard, Barkai shows that major investment in rearmament was not postponed until unemployment had been ended but had already begun before the end of 1934. Industrialists enjoyed internal autonomy in the production process, as well as strong financial incentives to produce, but had no voice in general policy, priorities or investment, and the use of profits was carefully regulated. Though this system was not the result of any clearly defined new theory, it had a rationale and a purpose and, in fact, worked remarkably well. The goal was not the highest efficiency or the maximal productivity but stability and the achievement of state priorities—goals that were fully attained.

Was the economic system of National Socialism capitalist? Barkai says *yes*, in that the ownership of most means of production remained theoretically in private hands; but it was not a capitalism dominated at the policy-making level by capitalists or one in which they enjoyed much freedom.

For the new, English-language edition of this book, Barkai has added a lucid 18-page chapter on the wartime economy from 1937 to 1945 to provide a fuller perspective. Most of the monographic research on Nazi economic administration still remains to be done, but Barkai's work has been the first to delineate accurately the economic doctrines of National Socialism, place them in the German ideological context, and relate them to the initial policies and administrative structure of the regime. It remains a major contribution to the history and analysis of National Socialism.

University of Wisconsin, Madison STANLEY G. PAYNE

A Treatise on Social Justice, vol. 1, Theories of Justice. By Brian Barry. Berkeley: University of California Press, 1989. 428p. $55.00.

This first volume of Brian Barry's projected three-volume treatise discusses the two dominant approaches to social justice in the Western tradition. The first runs from the sophists through Hobbes to theorists like David Gauthier. It understands justice as a contractual agreement among self-interested individuals seeking mutual gain, as contrasted to a nonagreement world or state of nature where egoists engage in untrammeled competition. The second position flows from the stoics through

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Kant to theorists like John Rawls and argues that justice emerges from an impartial point of view that does not make explicit reference to one's own advantage.

This volume compares justice as mutal advantage with justice as impartiality through an intensely rigorous survey and critique of the last 30 years' discourse on social justice. It focuses upon game-theoretic discussions of "fair division," utilitarian theories, and the work of David Hume and John Rawls. Barry addresses theories of social justice that assess basic institutions and the distribution of benefits and burdens over which individuals conflict such as rights, power, and scarce resources." The volume ends with Barry's own formulation of "the circumstances of impartiality." Volume 2 will elaborate and apply justice as impartiality, while Volume 3 will apply Barry's approach to wealth and income distribution.

Part 1 of the present volume analyzes "the so-called problem of fair division" as the basis of justice. This analysis revolves around a lengthy discussion of R. B. Braithwaite's formulation of how two musicians with differing utility curves can divide playing time "fairly." Barry points out that all mutual consent theories are driven by the assumed motive that individuals will seek to escape a nonagreement point that makes them worse off. Consent theories also claim that only self-interested agreement will give justice any stability or plausible chance of being accepted, especially by those with great resources.

Barry convincingly argues that all such theories generate outcomes that replicate or aggravate morally contingent inequality among parties. He demonstrates how these theorists, especially Gauthier, try to mitigate this unequal outcome by eliminating threats or imposing "irrational" strategies on individuals negotiating the hypothetical contract. But these arguments lack consistency with their basic premise.

The treatise also finds interpersonal utility an insufficient foundation for a theory of justice in either utilitarianism or "fair division" terms. This approach, says Barry, can be distorted by strategic misrepresentation and is built upon unstable claims. He concludes, "We must surely regard as suspect any solution that could be put into effect accurately only by Almighty God, unto whom all hearts are open, all desires are known, and from whom no secrets are kept" (p. 100). Instead, he proposes a "resourcist" solution, which defines issues in terms of the distribution of the resource itself, not the utility it generates. He believes that this approach will be more practical, plausible, and potentially egalitarian.

Part 2 shifts to Hume and Rawls and argues that they attempt an unstable compromise that seeks to unite mutual interest and impartiality. In both theories, the logic of mutual interest combined with the requirement to accommodate an unequal baseline undermines the claims derived from impartiality. Generally, Barry suggests that "circumstances of justice" require that original positions or veils of ignorance be devices to manipulate information and uncertainty to make justice "rational" for egoists. Barry believes that John Harsanyi has correctly shown that this logic drives individuals to average utility, not a Rawlsian difference principle. He believes Rawls must constantly "thicken" his original position in later works to account for this until the limitations on information recapitulate the conclusions Rawls wants.

Barry is committed to Rawls's egalitarianism, but believes Rawls's power derives from rejecting the moral relevance of contingent inequalities. But Barry claims that Hume and Rawls demonstrate that one cannot blend impartiality and mutual interest into a unified theory. The mutual benefit side constantly undercuts the demands of impartiality. This becomes evident in Rawls's difficulties in extending the theory to future generations and international relations.

Finally, Barry articulates his own position in part 3. He builds on what he calls a "constructivist" account of justice, where a theorist creates a "choice situation" specifying the power, knowledge, rules of reason, and other aspects bounding the contractors. The principles that emerge from deliberation under the conditions of the choice situation count as just.

Barry's choice situation is not hermetically sealed but interacts with other human intuitions about justice and requires an active acceptance of reasons by others. In his boldest and most fundamental move, he rejects self-interest as the only stable motive for justice. Human psychology and history suggest that humans possess a "moral motive" to justify their actions in terms that do not reduce to power or their own advantage. His conditions of impartiality postulate individuals who know their identity but seek principles "reasonably acceptable" to all. This account presumes what he calls "human decency" and moves justice from a game to a debate, with actors working to convince others with reasons that all can accept. Impartiality takes on an active political and democratic cast with a requirement that contractors accept the reasons. Justice also takes on less exactitude and deductive power than Rawls or utilitarians would prefer. It opens the possibility of a range of "just" decisions made within a broad framework determined by impartial deliberation.

This is a rich and dense book written with aplomb and technical care. As a treatise, it not only presents Barry's theory but also explicates and critiques substantial accounts within the field in a manner that invites further debate. It makes accessible an often arcane but influential set of reflections of the last 30 years and consolidates the insights of game theory, utility, and original position ideals of justice. Several technical appendixes are addressed to specialists; but generally, the book unites passion, technical competence, and rigor.

In one particular aspect it serves as a model of how intellectual debate should occur. Barry's careful, respectful, and relentless study and critique of Rawls's theory leads to the refinement and reformulation of Barry's own theory. His analysis of Rawls teaches how theoretical engagement can be done right. Political theorists interested in justice will find it illuminating and helpful as it places the debates of philosophers, economists, and game theorists within the tradition of political theory. By readmitting complex human motives and supple notions of reason, Barry articulates an approach to social justice for politics, not just for philosophy and economics.

University of Washington J. Patrick Dobel

Religion in Public and Private Life. By Clarke E. Cochran. New York: Routledge, 1990. 226p. $49.50 cloth, $15.95 paper.

This volume explores two themes: the nature of public and private life and the role that religion should play, particularly in the public sphere. The argument is essen-

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tially a prescriptive one, set out mainly within the context of the U.S. experience. Thus, within that culture, Cochran posits a distinction between a public and a private world, each "possessing distinctive features" yet, at the same time, "not radically separate. . . . These spheres intersect, forming regions of overlap and border territories" (p. 3). The import of religion is that it is one of the major social experiences inhabiting both spheres, especially that crucial area of overlap. On the one hand, it has a private face—the private and personal, most emphasized by Protestant understandings. On the other, it has a public face—the corporate challenge to participate in wider communal endeavors.

This, in turn, brings religion into the political domain, which is a major part of the public sphere. A tension is set up between the two, each—religion and politics—pushing against and challenging the other. Religion's role, therefore, is to connect the public and the private—to build up from private individual experience into the public world of politics and power to which it is fundamentally drawn. In that world, religion "contests . . . relativism by advancing in public debate principles claiming sacred roots" (p. 159). By speaking of justice and peace in a sphere largely governed by self-interest, it draws "politics beyond the level where it would otherwise settle" (p. 163).

In making these arguments, Cochran usefully contests—and rejects—the idea that God is to be restricted to the sphere of religion, private morality, and personal piety. He also, in advocating a *tensional model* between religion and politics, shows that religion can and should be taken seriously in politics—that politics needs the moral challenge of religious belief. It is a stance that rejects the idea of a "civil religion" (in which religion provides a moral foundation for the culture and the state), as well as theocracy. Equally, the relationship cannot be appropriately conceived, he suggests, through notions of secularization or the institutional separation of church and state. In short, he skillfully sets out a particular vision of the way in which religion should play a significant political part.

Two observations, however, are in order. First, perhaps too much of the book was spent in teasing out the public–private distinction to the detriment of working out in detail the tensional model of religion and politics. Some of this there is—on abortion, for example. But, I'd like to have seen more illustration—not just instances where conventional ideas about morality might seem most relevant, like sexual and family issues, but also in other areas like economic and foreign policy debates. This would help emphasize that valid point Cochran makes—that politics can and does teach religion the "realities" of life. It would also help clarify more precisely what Cochran means when he suggests that religion is there to help politics move beyond power and (self-)interest to discover a sense of adventure and a sense of worth.

Second, the argument is largely written against the backdrop of the U.S. experience; yet the issues of public versus private and religion versus politics are apparent in other settings. An exploration of some of these experiences and debates would have been appropriate. One limited but useful step, for example, might have been the consideration of the situation in England, which contributed considerably to the U.S. cultural and institutional framework. Not only, of course, is there an established Church of England representing a very different normative resolution of religion's public claims within liberal democracy, but there is also a substantial prescriptive literature (e.g., Ecclestone's *Church of England and Politics*, 1980; Preston's *Church and Society in the Late Twentieth Century*, 1983; and Temple's classic *Christianity and Social Order*, 1976). Such a broader canvass would have put the argument in greater relief. Perhaps this merely calls for a follow-up volume! If it does, it is a testament to a stimulating effort at clarification and advocacy.

University of Vermont GEORGE MOYSER

History, Power, Ideology: Central Issues in Marxism and Anthropology. By Donald L. Donham. Cambridge: Cambridge University Press, 1990. 242p. $49.50.

This study alternates between general theoretical discussions and ethnographic detail in a particularly fruitful fashion. Donham defines neoclassical economics as minimally characterized by a claim that preferences can be unambiguously ranked and a focus on individual maximizing activity within the context of a given preference structure that remains stable in the short term. Thus, Donham claims that neoclassical theory has wide applicability in time and space at the same time that he argues that it is quite incapable of either significant historical explanatory power or of addressing issues of power and ideology. These critical areas require an analysis of both historical contingency and structural causality. The book illustrates this theme through an analysis of Maale ethnographic data, particularly data on productive labor, including that of cooperative work groups, leavened by dialogue with anthropological and economic theory.

Donham makes a number of brilliant contributions to Marxist theory. These can best be appreciated by contrasting Donham's positions with those of scholars whose work has had the greatest formative influence on his argument. Donham's starting point is Gerald A. Cohen's argument that the superstructure provides essential stability for the base (*Karl Marx's Theory of History*, 1978). Donham pushes this position a little further, suggesting that the superstructure is in fact the sine qua non of *productive inequalities* (p. 195; his brilliant translation of Marx's *Produktionsverhältnisse*, usually glossed as "social relations of production"). Donham follows Cohen in arguing that Marxist explanations of this link are fundamentally functionalist ones. In doing so he uses Raymond Williams's notion of *epochal analysis*, (*Marxism and Literature*, 1977) but tries to push this idea, too, a little further. Rather than circumscribing the term to the analysis of dominant, residual, and emergent cultural processes (constituting the essential ties to history of a specific epoch), Donham argues that a similar conceptualization of patterned space, with respect to power, is also required, on the lines of Immanuel Wallerstein's *Modern World System* (1974) or George W. Skinner's The *City in Late Imperial China* (1977). More generally, he argues that the concept of *articulation* relies too much on functional ties (his key example is Claude Meillassoux's *Maidens, Meal, and Money*, 1981) and so presupposes an exaggerated concurrency in time and place. His solution is to combine insights from Edward P. Thompson, who argued that epochal analysis bypassed the chaos of history, (*"The Poverty of Theory" and Other Essays*, 1978),

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with Cohen's functional interpretation applied to epochal analysis. In brief, Donham suggests that epochal analysis reveals how the die of history is loaded—how productive inequalities are arrayed in a given point in time—but cannot fully explain origins of, or transformations in, epochal structures. To do so would require, in addition, a broader historical approach and an appreciation of how historical changes would lead to changes at the level of individual maximizing behavior.

On the ethnographic side, the first contribution of the book is to provide a particularly persuasive analysis of the interconnections between Maale household dynamics, Maale cooperative work arrangements, and traditional justifications for productive inequalities favoring male elders—an ideology based on a fetishism of fertility (rather than of market commodities). The second contribution is to link this analysis both to structures of domination of national and international scope and to historical processes not adequately circumscribed by such units of analysis. Donham's method of choice is to begin with a puzzle: contra Aleksandr Vasilevich Chayanov (*The Theory of Peasant Economy*, 1966), households do not seem to increase their productive work as the ratio of household consumers to household workers increases. Further, contra Marshall D. Sahlins (*Stone Age Economics*, 1972), these discrepancies cannot be explained by redistribution of agricultural products. The solution to the puzzle is that senior households provide labor (of young men in their household), not agricultural products, to households with high needs relative to productive capacity and do this to maintain their social and economic standing as guarantors of fertility in the face of the socialist principles of the 1974 revolution and the antitraditionalist principles of newly converted protestants. Donham is particularly convincing when he suggests that cooperative work arrangements potentially combine to link the population in a variety of ways, the 1975 system being a recent response quite different from that pertaining to the period from the late nineteenth century to the eve of the revolution. Donham's analysis suggests, moreover, that the dominant configuration, defining the basic productive inequalities in Maale society, would more generally have varied significantly over both time and space. Rather than providing a contrast between modern capitalist and traditional noncapitalist economies, Donham suggests that there are no qualitative socioeconomic differences justifying qualitatively different theoretical approaches. He suggests that the other is, indeed, us if properly understood (p. 15).

This book is highly recommended not only for all Africanists but for all readers interested in political economy in its widest sense.

University of Arizona THOMAS K. PARK

The Unvarnished Doctrine: Locke, Liberalism, and the American Revolution. By Steven M. Dworetz. Durham: Duke University Press, 1990. 247p. $37.50.

A Union of Interests: Political and Economic Thought in Revolutionary America. By Cathy D. Matson and Peter S. Onuf. Lawrence: University Press of Kansas, 1990. 237p. $25.00.

It was not that long ago that the Hartz thesis still commanded the loyalties of U.S. scholars who were convinced that the United States was imprisoned by a liberal consensus where Lockean ideas held undisputed sway. However, as Stephen M. Dworetz observes in *The Unvarnished Doctrine*, scholars have abandoned Hartz with astonishing speed and, in place of Lockean liberalism, have substituted an equally omnivorous civic republicanism, which now seems to include everyone except Locke (p. 5). Thus, it is refreshing to find two books arguing that the republican revision is now in need of revising—and from a liberal perspective, no less!

Dworetz's book is important for both historiographic and political reasons. On a historiographic plane, Dworetz conducts "a critical examination of the republican revision and a reassessment of the role of Lockean–liberal ideas in the American Revolution" (p. 7). Dworetz reexamines revolutionary pamplets, election sermons, and newspaper essays to find that a reading of the texts "testifies consistently and often explicitly" to the importance of Locke's liberalism and to his revolutionary stature in the minds of American colonists (p. 8). Thus, Dworetz's historiographic objective is to restore Lockean ideas to a position of importance in the American Revolution and to suggest, at a minimum, that Lockean liberalism coexisted with (or, perhaps, was a part of) the republican ideology. Indeed, Dworetz concludes that American revolutionary ideology is best understood as "liberal republicanism."

However, Dworetz considers the rehabilitation of Locke important for political reasons that extend behond scholarly polemics. Dworetz points out that in the United States "the dominant understanding of the founding doctrine . . . contains prescriptive implications for public policy and constitutes the essential source of historical legitimacy for any general political program" (p. 4). In this context, Dworetz correctly calls attention to the seamy underside of civic republicanism: "Militarism, imposed social and religious conformity, and even American slavery are known to have been justified in the language of civic republicanism" (p. 5). Consequently, Dworetz fears that by expunging Locke from the founding ideology "the reinterpreters exclude from the ideology of the American revolution, or radically demote therein, the one tradition of political thought that *inherently* authorizes religious toleration, constitutional politics . . . and popular revolution against actual or intended tyranny" (p. 4). On these points, Dworetz's work hints that the emergence of "republican" historiography parallel with the rise of the Repulican party is more than linguistic synchronicity. Behind the scholarship of Bernard Bailyn and J. G. A. Pocock lurks the politics of George Bush, William Bennet, and Jesse Helms. Hence, Dworetz seeks "to persuade the democratic critics of liberalism to think twice before jumping onto the bandwagon of American republicanism" (p. 193, n. 2).

On a much broader front, Cathy D. Matson and Peter S. Onuf argue in *A Union of Interests* that while American revolutionaries and early state builders "naturally found republican ideas attractive," the republican political discourse disguised "significant 'liberal', or progressive tendencies in American political economic thought" (p. 11). Echoing Dworetz's theme, Matson and Onuf also challenge the republican synthesis by pointing out that "in historiographical retrospect, the contradictory tendencies in American revolutionary thought and practice appear striking" (p. 11). The authors utilize an impressive array of primary sources to illustrate how U.S.

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political economists "drew on classical republican, liberal, and jurisprudential traditions, among others, to conceive and construct solutions to the problems they saw themselves confronting in the aftermath of the Revolution" (p. 2).

The authors' main thesis is that a clash between classical republicans and liberal nationalists was precipitated by a "crisis of the union . . . defined in economic terms" (p. 2). Matson and Onuf argue that during the critical period a variety of "commercial interests" became increasingly dissatisified with the Confederation and began to criticize it from a nationalist perspective. According to the authors, the nationalist movement was initially based in international shipping and trading interests, a coalition that included U.S. shippers, commercial farmers (exporters), and public creditors (pp. 45, 70–71, 147). These groups were soon joined by a "manufacturing interest" consisting of merchants and wealthy mechanics, who then enlisted the support of urban artisans and small farmers engaged in household manufactures (p. 46). Finally, parties interested in the development of the Western lands joined the coalition, including Southern planters and Western farmers seeking a government that could maintain free navigation on the Mississippi River.

At the same time, the authors contend, the critical period exposed a latent tension between virtue and interest in republican ideology. As they note, the republican view of political or moral economy "was founded upon the premise that political right and personal virtue flowed from the ownership of hereditary and transmissible land" (p. 12). However, in the United States, "the idealized republican yeoman too often proved to be among the most rapacious and greedy patriots, not the most virtuous" (p. 36). The agrarian cornerstones of republican ideology were more interested in farm exports and the price of public securities than in virtuous citizenship.

According to Matson and Onuf, what emerged from the confrontation between traditional republicanism and a rising liberalism was the discourse of U.S. *federalism*, in which the ideological disputes between liberal nationalists and classical republicans were finally narrowed to divergences over the *scope and scale* of a federal republic. The authors demonstrate how a distinctive political economy of federalism was forged by a gradual melding of these interests in pamphlets and newspaper essays. By the time *The Federalist Papers* were published, most of the federalist ideas were already in wide circulation and pointing toward "a union of interests" structured by a vision of national economic development. Matson and Onuf thus rightly conclude that the genius of the Federalists was to link ratification of the Constitution to the rapid development of a national economy "so as to appeal to the many optimistic and enterprising Americans who hoped to reap the rewards of independence" (p. 2).

Southeastern Massachusetts University CLYDE W. BARROW

Topics in Social Choice: Sophisticated Voting, Efficacy and Proportional Representation. By Dan Felsenthal. New York: Praeger, 1990. 208p. $47.95.

Dan Felsenthal uses the tools of social choice theory to evaluate three topics overlooked in most of the literature. Part 1 explores the effects of assuming that voters use sophisticated strategies in simultaneous voting arrangements; part 2 evaluates voter efficacy in small electorates and under different voting rules; and part 3 applies a social choice framework to proportional representation schemes. While it considers some important extensions to the existing literature, this book serves best as a supplement to, rather than a replacement for, core texts.

Part 1 incorporates theoretical and experimental analyses of sequential sophisticated voting strategies and provides the most complete response to any of the three issues raised. Using a two-by-two design, Felsenthal compares plurality and approval voting schemes under sophisticated and sincere strategies to determine to what extent either voting mechanism is vulnerable to the undesirable properties associated with sophisticated behavior as defined in the models of Robin Farquarson (*Theory of Voting*, 1969) and Niemi and Frank ("Sophisticated Voting under the Plurality Procedure," in *Political Equilibrium*, ed. Peter Ordeshook and Kenneth Shepsle, 1982). He finds that approval voting does somewhat better in terms of Condorcet efficiency but that both procedures violate a series of socially desirable properties when sophisticated strategies are used. In response to these findings, Felsenthal develops a new behaviorally consistent model of sophisticated voting based on tacit cooperation among supporters of a Condorcet winner. Laboratory experimental tests comparing the tacit cooperation model of sophistication with Farquarson's, Niemi and Frank's, and sincere voting models find support for the tacit cooperation model. This is an important contribution in two respects. First, one of the major criticisms of models assuming sophisticated strategies is that the voter must presume that other voters are always pursuing a sophisticated strategy when one is available—an assumption that is difficult to meet in natural settings. By assuming a sophisticated strategy in which voters should vote sincerely under certain conditions and sophisticatedly under others, Felsenthal's model allows voters to assume a middle ground in which sophistication is used when it would benefit voter's interests. Second, many previous empirical studies find relatively low levels of sophistication, a result that is troubling, given the assumptions of social choice. By redefining sophisticated strategies in this way, Felsenthal incorporates a broader set of behaviors under the banner of sophistication and shows that voters behave sophisticatedly more often than has previously been demonstrated in empirical tests.

The question of how effectively voting mechanisms translate an individual's preference into social choices is at the heart of social choice analysis, and Part 2 explores this question under two rather unusual voting arrangements and under conditions where results are sensitive to the size of the voting population. Plurality and cumulative voting systems are compared and found to produce similar results, suggesting that the added difficulty associated with a cumulative scheme is unjustified. Felsenthal evaluates a combined approval/disapproval voting system to find that it provides greater voter efficacy than the approval mechanism alone, particularly when the number of candidates is large.

One of the most interesting chapters contains an evaluation of how sensitive plurality outcomes are to different quorum rules. While the results presented in this section are individually interesting, their overall

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impact is limited by the fact that their tie to the larger literature is left largely to the reader to devise. This tie-in is especially important, considering that the voting rules examined are relatively unusual. The reader would have benefited had the efficacy of these voting arrangements been placed in the context of other studies of voter efficacy and the implications of the results been drawn out more carefully.

While many democratic electoral systems rely on proportional representation, social choice has paid relatively little attention to this form. In an effort to correct for this oversight, Felsenthal develops a normative standard for the distribution of seats in proportional representation schemes using the social choice tehcniques originally developed to deal with single-representative systems. Again, the emphasis is on the link between voter preferences and social outcomes; and three different methods of seat allocation are developed and compared. These methods are tested empirically against plurality and approval voting schemes using the 1985 Israeli elections and are found to produce seat distributions falling between those produced by plurality and approval voting rules. Since plurality systems tend to overdistribute seats to leading parties and approval schemes tend to underdistribute to these parties, these other methods deserve additional consideration as possibly improving the link between voter preference and representative outcome.

The major strength of this volume is that it combines abstract theoretical results with a wide range of empirical tests including computer simulations, laboratory experiments, and field experiments. The variety of presentations provide some material that should appeal to a broad audience in social choice. Two factors, however, limit the book's usefulness. First, there is little to tie the three parts of the book together other than the fact that each deals with a topic somewhat overlooked in most social choice analysis. The author does not provide a synthesis to suggest what the impact of this book's contribution might be for reevaluating the relevance of social choice approaches. The result is that the three parts do not add up to a single coherent volume. This problem is exacerbated by the fact that several of the individual chapters are based on collaborative work and the author has failed to edit sufficiently to remove style differences and inconsistencies among the treatments. Second, the book is based on work that almost without exception has appeared in print in other venues. This would not be troubling if the synthesis in this volume resulted in a product exceeding the sum of its parts. However, often the task of linking the results together is left to the reader; hence, packaging these papers in a single volume does not have the impact it should have.

Indiana University ROBERTA Q. HERZBERG

Nietzsche's New Seas: Explorations in Philosophy, Aesthetics, and Politics. Edited by Michael Allen Gillespie and Tracy B. Strong. Chicago: University of Chicago Press, 1988. 240p. $24.95 cloth, 13.95 paper.

Nietzsche and Political Thought. By Mark Warren. Cambridge: Massachusetts Institute of Technology Press, 1988. 311p. $30.00 cloth, $12.50 paper.

"To improve one's style—means to improve one's thoughts and nothing else!" Nietzsche wrote in *The Wanderer and His Shadow*, priding himself on being both thinker and literary stylist. His goal, so far as it may be summarily defined, was to investigate what the life of a modern "philosopher–artist" entailed and to convince others that living this life was, at the least, highly desirable. Nietzsche's aphorism concludes, "If you do not straightaway agree with this it will be impossible to convince you of it." Here we discover Nietzsche's aristocratic bent, often reviled but also intrinsic to his thought. Nietzsche wrote for a restricted audience because he believed there were few with ears to hear his words. His readers already would have to be philosopher–artists in spirit to understand and appreciate his literary–philosophic creations. *Nietzsche's New Seas* attempts to address the nexus between Nietzsche's philosophy and his art in a manner consistent with Nietzsche's own understanding of the importance of style. It is a stimulating collection of essays. However, certain contributors neglect the import of the first part of Nietzsche's aphorism. Great style is always only the child of great thought. The two may become inseparable in practice, but the prerogatives of parentage should be maintained. Very different in approach and intent, *Nietzsche and Political Thought* incisively identifies, and then purges, those aspects of Nietzsche that are socially exclusionary. While not unaware of the place of style, the book is dismissive of its centrality and unconcerned with its function in the systematic quest for a Nietzsche that accords with its author's politics. Here, the importance of the second half of the aphorism is neglected. For one may legitimately separate Nietzsche's philosophy from his politics; but it is Nietzsche's philosophy, not his politics, that is selective of its audience and intrinsic to his art.

While the nine essays of *Nietzsche's New Seas* are diverse in their approaches, a central concern is to be gleaned from all. What is new both in Nietzsche's philosophical navigations and in these attempts to navigate around Nietzsche's philosophy is a sensitivity to form. The form of interpretation practiced in this collection is different from all forerunners, the editors hold, because "in this new incarnation, Nietzsche no longer appears primarily as the prophet and purveyor of nihilism but as the thinker who marks a kind of ending to, or at least a rift in, the continuity of the West" (p. 1). In what might appear as a kind of rift in the editors' opinions, however, we learn a few pages later that the new approach represented by these essays is nonetheless predicated on the "centrality of nihilism" to Nietzsche's thought (p. 5). The confusion is resolved by viewing Nietzsche's radical break in and break-up of Western culture as the product of his pronouncements on nihilism. Unlike previous philosophers, Nietzsche did not ignore or reject nihilism. Instead, he explored its historical depth and celebrated its "active" form with music and poetry. The celebration of the poetic and musical Nietzsche in these essays, therefore, is also a celebration of that "active nihilism" Nietzsche identified with the death of God and the subsequent demise of God's shadows, metaphysical truths. Once philosophy no longer considers itself metaphysical and foundational, it becomes—and perhaps must become—aesthetic.

To my mind, the highlights of this volume are essays by Karsten Harries, Sarah Kofman, and Tracy Strong. Strong persuasively argues that Nietzsche's concern is with aesthetic self-transformation, not political domination. Nietzsche's fear that he had reached the limits of

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what could be achieved through aesthetics, Strong speculates, pushed him into the realm of explicitly political reflections on domination and, perhaps, eventually pushed him beyond the realm of communicative thought. Kofman's essay also attempts to redeem Nietzsche, taking on his misogynist reputation. Nietzsche's ambivalence to women is well demonstrated by Kofman, as is the philosophical importance and literary richness of his notion of the feminine. Kofman asks, in conclusion, if Nietzsche's polemics "are not symptomatic of a deep love of women, all of whom had abandoned him when they might have served him as a lightning rod . . . or even an umbrella?" (p. 199). As insightful as Kofman's essay proves (and his ironic humor notwithstanding), one must question whether trading Nietzsche's distasteful but revealing chauvinist vitriol for an insidious partriarchal gratitude constitutes a worthwhile transaction. In "The Philosopher at Sea" Harries discerningly explores Nietzsche aphoristic literary style, finding its fractures conducive to Nietzsche's philosophic purpose. By way of his artful lure Nietzsche kidnaps his readers for a philosophic voyage in uncharted and choppy waters.

Michael Gillespie's essay, on the other hand, attempts to demonstrate more method to the master's madness, disputing the belief that Nietzsche's philosophy is fragmentary. The claim is that much of Nietzsche's later work (explicitly, the *Twilight of the Idols*) is systematically structured according to the form of the musical sonata, with literary and philosophical equivalents of cadences, keys and harmonic changes. Gillespie's thesis is highly original but quite unfounded. The analysis opens with the claim that the preface to *Twilight of the Idols* refers to the time signature of a sonata (3/4 time, or triple meter): "The preface is divided into two sections separated by a line of Latin. There are 135 words above and 180 words below the Latin line, which constitutes an exact proportion of 3/4" (p. 124). The author notes that Nietzsche's musical intent, here revealed through a numerological puzzle, "might seem far-fetched" (p. 148). Indeed, its dubiousness is certified once one notices that Gillespie miscounted his words, there being 181 not 180 below the Latin line. Failing the possibility that Nietzsche also miscounted, Gillespie's "musical politics" proves rather offbeat. What is salvageable from the essay is the claim that *Twilight of the Idols*, like much of Nietzsche's work, has a discernible structure. That this architectonic quality is loosley shared with musical sonatas (and, one might add, with most books, essays, lectures, films and various other forms of musical composition) is unsurprising.

Contrary to *Nietzsche's New Seas*, Warren's *Nietzsche and Political Thought* is premised on the assumption that Nietzsche's style is not a sufficient focus of study—"nor for that matter is it even a good way of entering Nietzsche's thought" (p. xii). The book is a finely crafted argument detailing how Nietzsche, despite his politics of "neoaristocratic conservatism," may be employed for the purposes of constructing a postmodern (read non-metaphysically-grounded) politics of democratic egalitarianism, reciprocity, and social care. The key to squaring this circle is the rejection of Nietzsche's "premodern" socioeconomic assumptions and remaining metaphysical beliefs that caused him to "read domination back into his philosophy of power in ways that violated the internal integrity of his critical, postmodern insights" (p. 3). This distillation of Nietzsche is then combined with the reworking of his thought to the point where Nietzsche's central concern is determined to be the development of human agency, inherently a political category defined as the capacity to make decisions, initiate projects, and enter into reciprocal obligations.

Nietzsche announced that he sought a "Caesar with Christ's soul." Warren seeks a Nietzsche with Marx's soul. This Nietzsche displays an overriding concern for the problems of social injustice, violence, and development. He is, above all, a social critic. Nietzsche the poet of tragic heroism and philosopher of radical individualism, who plumbed the depths of his soul that he might raise himself above and beyond social norms and constraints, is scarcely to be found in these pages. The Nietzsche that emerges from Warren's book, then, is indisputably a retreaded one. In itself, this is an unavoidable result of any imaginative interpretation; and Warren is rightfully unapologetic for his project. But the question remains as to whether the retreading was so extensive a piece of work as to make the substratum unrecognizable, or whether the core is so thin that despite the craftsmanship displayed in its renovation the product is likely to fly apart when put to the test. Warren's effort is laudable for its rigor and scope. Yet I believe he remains unsuccessful in the very worthwhile task he set himself, namely, "to show how one might choose the philosophical Nietzsche while excluding his politics, but to do so without eclecticism—that is, without fragmenting the internal necessities of his thinking" (p. 208).

University of Florida LESLIE PAUL THIELE

Theory in Practice: Tocqueville's New Science of Politics. By Saguiv A. Hadari. Stanford: Stanford University Press, 1989. 182p. $27.50.

Tocqueville continues to attract and to inform, whether as historian and (sympathetic) critic of democracy or prophet of a "new world." His descriptions, insights, and predictions are like nuggets—promises of even more substantial findings below the surface. *Theory in Practice* plumbs the depths in search of the core.

That yet unearthed treasure is Toqueville's methodology. In Tocqueville we have an illustration of social theory not only rigorous, impartial, and therefore scientific but contextually appropriate, interpretive, and practical. As Saguiv Hadari portrays it, Tocqueville is able to achieve in theory what modernity is unable to achieve politically—the best of both worlds. Describing it in one place as "successful eclecticism," Hadari concludes that "Tocqueville's approach qualifies as both 'economic' and 'sociological', and fuses together 'classical' and 'modern' perspectives" (p. 5). In Tocqueville, then, we have a proven alternative, an answer to the multiple, often incommensurable social theories of today.

This is not to say, however, that Tocqueville necessarily rejects—or that he simply transcends—contemporary dichotomies. (Hadari lists, among these, nomological vs. particular, macro vs. micro, formal vs. interpretive, and objective vs. subjective approaches.) Hadari argues something quite different and contentious, namely, that Tocqueville traverses these dichotomies while still (in some sense) recognizing them, without obliterating them.

Hadari works from the position (also contentious) that Tocqueville's *Recollections, The Old Regime and the French*

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Revolution, and *Democracy in America* form and reflect an integrated whole. He finds (in part 1 of the *Recollections*) Tocqueville to be "grounded in abstract, nomological models of behavior" (p. 13), including those of a "game-theoretic" and/or rational choice nature. In his part 2, however, where Tocqueville's study of the French Revolution is emphasized, Tocqueville is revealed as the theorist of "self-understandings." Wishing "to penetrate to the heart of this old regime its ideas, its passions, its prejudices, its practices," Tocqueville's recourse to hermeneutics is unavoidable and the "key to the originality and insightfulness of *The Old Regime and the French Revolution*" (p. 65).

Democracy in America (part 3) reveals Tocqueville as normative, that is to say, striving to provide "practical orientation about what is right . . . without relinquishing . . . the rigor of scientific knowledge" (p. 116). Here Hadari is at pains to portray Tocqueville's approach as one where "'objectivity' does not mean value-free, strictly descriptive analysis" and "normative" does not imply "a lapse into merely subjective evaluations" (p. 116).

Theory in Practice presents a close and detailed argument. It is advised that the reader have a working knowledge of Tocqueville's major works and of the methodological issues of contemporary social theory.

Emporia State University THOMAS CLAY ARNOLD

Property Rights and Poverty: Political Argument in Britain, 1605–1834. By Thomas A. Horne. Chapel Hill: University of North Carolina Press, 1990. 296p. $34.95.
The Realm of Rights. By Judith Jarvis Thomson. Cambridge: Harvard University Press, 1990. 383p. $35.00.

Ours is a rights-centered culture, and our political dialogue is largely framed in the language of rights. But what it means to have a right is not always clear. Clarity about the language of rights is important because theories of rights generally impose the duty not to interfere with the rights of others. For some, questions about what constitutes a right and who is a rights carrier are easily answered. Others, however, find a lack of continuity in the way we assign these terms. One route the controversy has taken pits claims for human dignity against claims for the unrestricted use of private property.

In very different ways, Thomas Horne and Judith Thomson prove to be important and lively contributors to the debate about the status of rights. Horne offers a strong and enlightening discussion of the rights and obligations connected with property ownership in early British theory, and Thomson builds on Hohfeld's contributions to solve some vexing puzzles about rights. Each clears away cumbersome impediments to clarity and invites readers to think freshly about the language of rights.

Thomson argues that one class of rights claims stems from a person's humanity and creates a strong collateral harm principle that cannot be lightly breached. We can expect rights to be respected (on her account) because human beings can behave according to moral laws, and, that being the case, others have claims against us to respect their rights. A different class of rights claims stems from social practices and law and is derived from promising and consenting in particular contexts. By this line of reasoning, private property rights claims are conditional on a legitimate legal system and cannot be considered a natural right.

For Thomson, rights are best understood as clusters of rights, privileges, and duties. What emerges is not a system of rights where one cluster invariably trumps another but a discussion about the *Realm of Rights* where different people, given their varying social circumstances, produce different clusters of rights, privileges, and duties. The array of rights that one person carries may not be the precise array of another person; and the rights a particular person holds at one time may not necessarily be the same rights the person will hold at a later time. Nevertheless, for Thomson, this ought not be taken as a denial of the first category of rights (resembling natural rights), which is something that cannot be altered at its core even if its expression may vary in practice.

The strength of Thomson's work is to teach us to think analytically about rights. At the same time, though, this approach only has a limited political impact. The legal and political systems that play such a large role in her second form of rights often reflect configurations of power that serve some individuals while ignoring the claims of others. But rights are thought to be separate from power, not dependent on it—a subject largely neglected by Thomson.

Horne is primarily concerned with Thomson's second category of rights. He shows how classical liberal theories on property contained both an inclusive and exclusive (or common and individual) dimension and how several writers attempted to accommodate both aspects. Beginning with Grotius and Pufendorf, and continuing through Locke, Smith, Bentham, and Paine, Horne demonstrates the diversities and continuities of property rights in liberal theory and argues that the modern welfare state has its roots in early liberalism.

Some of the authors Horne surveys hold that in cases of individual necessity, the poor are given valid claims on the property of others for their own survival. Sometimes, the right of survival by the propertyless is transformed into a duty on the propertied to pay taxes that are used by magistrates to assist the needy. For others, the right of necessity gave way to a weak duty of charity by the propertied to the poor.

Many natural rights theories that emphasize the exclusive use of property simultaneously forge a strong argument for equality—and, according to Horne, for inclusive conceptions of property and even the welfare state. If people are credited with a natural right to rebel when their right to liberty is denied, what strong argument could deny them the right to the property of others when their own survival is at stake? In a similar vein, Horne also shows that early liberal theories that relied on individual labor to justify an exclusive rights claim to private property were converted by some later liberals to arguments that vast inherited holdings were divorced from labor and therefore subject to working-class claims for a greater share of national wealth. The authors most likely to avoid undermining a strong exclusive view of property were writers like Filmer and Burke, who rejected the egalitarian and rationalistic assumptions of liberalism.

Horne also reminds us that early theories of property commonly carried strong admonitions for moderation and warned about excessive ambition and avarice. However, such virtue is self-enforced. Just as charity is a highly individualistic way of addressing poverty, so moderation is a private disposition, not an institutional response to unlimited acquisition.

Horne's work is a remarkably compelling examination

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of liberal theory's many different conceptions of private property and of the duties of property holders to the impoverished. While he sometimes pushes his argument about the implication of charity on future liberal thinking too far, Horne shows that a simple reading of liberalism as a theory of acquisition and possession misses much more than it captures. He also demonstrates that many of the elements in the liberal–communitarian debate blur the recurring feature of most liberalism that property rights are encumbered and that property holders owe some (if only a scanty) responsibility to those who are the least well off.

University of Maryland RONALD J. TERCHEK

Situation and Human Existence: Freedom, Subjectivity and Society. By Sonia Kruks. New York: Routledge, 1990. 215p. $15.95 paper.

John Rawls tells us that a fair solution to the problem of justice must be acceptable to "free and rational persons." Michael Sandel responds that such persons do not exist except as members of a particular community. And a diverse group of postmoderns (e.g., Michael Foucault, Jacques Derrida, Pierre Bourdieu, Jacques Lacan, and Francois Lyotard) agree that free and rational persons do not exist anywhere and never will. Sonia Kruks takes up this issue, arguing quite correctly that it will not be resolved by assertion but only by the careful study of how "situated" men and women truly are. To what degree are we prisoners of the situations we find ourselves in—situations that range from being the possessor of a body upon which we depend, to being a member of a family, to belonging to a social class, to being a citizen of a nation? Surely this is a terribly difficult and important question—that is why it is so often dealt with by fiat.

To answer this question, Kruks turns to a diverse group of French existentialists (Gabriel Marcel, Jean-Paul Sartre, Simon de Beauvoir, and Maurice Merleau-Ponty), devoting a chapter to each, with the exception of Sartre, who gets one for the early period of *Being and Nothingness* and another for the later period of *Critique of Dialectical Reason* (which, Kruks argues, in effect rejects Sartre's earlier claims regarding absolute freedom). Hers is in the main a fruitful quest and a pleasant change from the faddish search for the latest and most extreme French thinker of the month. For what Kruks shows is that in spite of the considerable flaws of each, all (with the exception of the early Sartre) struggle with the relationship between situation and freedom, trying to come to terms with what Merleau-Ponty calls the "impure subject"; that is, the authors struggle with a concept of subjectivity that is neither the constituting consciousness of traditional Western philosophy nor the constituted consciousness of postmodernism. Such a concept conceives of the self as situated, embodied, intersubjective, and practical (while not reducible to any of these).

Kruks carefully and critically explicates the position of each author. In fact, she devotes so much attention to this task that the book risks becoming a sort of Cliffs Notes for the very advanced. This is particularly the case with her treatment of Sartre, since so much of what she rightly criticizes about his project has been noted by many others. It is less of a problem with her study of Marcel and Beauvoir, primarily because they have received less critical attention than they deserve, so that a summary of what they have received is more rewarding. So often, I had the feeling that Kruks knows more than she is telling. This is not the worst experience when reading a book—and certainly better than its opposite.

If Kruks's book risks becoming strictly a critical explication, it is saved from this fate by a short but dynamite conclusion, where she suggests that those who assert either the absolute freedom of the subject (early Sartre), or the subject's absolute unfreedom (Foucault) are holding to inherently unstable positions, at least if the holder of such a position also wishes to be a social critic. Foucault, for example, does not so much soften his antihumanism in several later works as simply revert "to the classical conception of the autonomous subject" (p. 188). How much more fruitful it is, concludes Kruks, to accept and struggle with the idea of the impure subject—neither wholly constituted nor entirely free but always moving between the two, depending among other things, on the subject's actual historical situation.

The existentialists' willingness to struggle with the issue of the situated self, rejecting the charms of extreme solutions and simplifications, seems to me to be their great contribution. Where they fail (the early Sartre most of all and Beauvoir probably the least) is in their tendency to "go ontological"—to write about the essence of being as though they were talking about real human beings. Consider the claim that being realizes itself in constituting the conditions under which it might come to apprehend its own subjectivity. It is a claim that I just made up and that (as far as I know) means nothing. Yet precisely because it is a claim about nothing at all, this is easily overlooked. Although Kruks knows this (I am sure that she would not put it this way), she sometimes lets her authors get away with too many statements of this kind. In the end, it is talking about the real lives of real men and women in real situations that keeps us honest, that is, keeps us involved with the reality of impure subjects—the only subjects there are.

University of Maryland, College Park C. FRED ALFORD

Rawls: "A Theory of Justice" and Its Critics. By Chandran Kukathas and Philip Pettit. Stanford: Stanford University Press, 1990. 169p. $22.50 cloth, $8.95 paper.

Kukathas and Pettit claim that justice as fairness has lost its nerve. Although the contractarianism of Rawls's *Theory of Justice* is puzzling (it is not as indispensable as some suppose, since once "the constraints of right are put in play, then most candidate principles for determining the basic structure of society must be judged to be infeasible"), still, "not only does the contractual device help us to identify what we would choose among basic structures—feasible and infeasible—if we were impartial," but it also dramatizes "the sort of thing that we should be looking for when we seek out the best basic structure for a society" (pp. 63–64). And that project at least involved argument—not merely accommodation and conciliation, which is all there is to the stability-minded "political liberalism" of the recent Rawls.

Strangely (given their mission of reconstructing Rawls's degeneration), the authors do not mention the Kantian interpretation of justice as fairness in *The Theory of Justice*; and the entire issue of Kantian constructivism is treated

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so gingerly (in the last chapter) that one would never guess from reading this book how or why the Kantian theme has loomed so large in Rawls's work throughout. This omission is remarkable not least because the book goes on to consider various critiques of Rawls's Kantianism at some length and, in a confused, ambivalent way, to lament his supposed abandonment of it.

Against Nozick, the authors rightly stress the importance of the basic structure and proceduralism, though they do not convey Rawls's own view of his approach as "quasi" or "pure-adjusted" proceduralism. Indeed, on the issue of what a just Rawlsian state should look like, they make the common blunder of suggesting that although "Rawls does not put it in these terms, it looks as if economic justice is given institutional expression by the organs of a welfare state" (p. 51). In fact, Rawls sharply denies that his theory is a defense of the welfare state. As the French edition of *The Theory of Justice* shows, the real contenders for just economic institutions are democratic socialism and "property-owning" democracy—not surprising, given Rawls's emphasis on production.

In reply to the communitarian charge that justice as fairness represents abstract, Kantian universalism, they go too far in claiming that the theory is bound to a particular historical context, namely, the U.S. democratic tradition: "In his Dewey Lectures (1980) Rawls explicitly acknowledges that his concern is not to supply a universal standard of justice but to discover those moral principles which might best serve his own society, with all its particular concerns" (p. 107). But crucially, Rawls's claim has only been that his theory *at least* applies to modern constitutional democracies; he simply wants to bracket many of the historical questions. (This agnosticism also figures in *The Theory of Justice* to a degree, though the issue is complex.) Still, he does want to claim that justice as fairness is "appropriately universal." The depth of its ideals is such that it is more than a public conception for the United States. It applies to many modern democratic states and is capable of being extended into a conception of international justice.

This is important because the confusions of these earlier chapters are aggravated in the final chapter, where the authors claim that Rawls's post-*Theory* work shifted to a more Kantian view (because of the more explicit appeal to a Kantian ideal of the person, supposedly at its peak in the Dewey Lectures)—followed, from about 1982 on, by an unfortunate shift to a more Hegelian or pragmatist view. The latter phase establishes the principles of justice "not by challenging and rejecting competing comprehensive conceptions but by trying, as far as is possible, to transcend and *accommodate* them. The point . . . is not to find the truth about the principles of public morality, but to ensure social unity and stability in long-run equilibrium by forging a stable agreement among the various moral, religious and other comprehensive philosophies" (p. 148). For Kukathas and Pettit, the idea that justice as fairness is a practical political view (one expressly framed for the basic structure of society and drawn from the public political culture, rather than from any particular comprehensive moral doctrine) marks the feasibility concern run riot. Rawls's contractarianism is "now re-interpreted . . . as a solution to the problem of finding a suitably feasible conception" (p. 143). The "constraints on rational choice" simply reflect the latent ideals of U.S. society; and the principles chosen in the original position "amount to a conception of justice which is stable because they are principles which would be chosen by that society. These principles are therefore feasible" (p. 143). Whether the principles of justice are "desirable" is now "clearly of subordinate importance" (p. 143). Rawls, they claim, has despaired of "reasoned debate."

Does this mean reasoned debate in one country, an all-American Kant? Given that the rot supposedly set in around 1982 (well after the Dewey Lectures), this would seem like the only conclusion consistent with the book's reading of the earlier Rawls unless the authors also mean to suggest that the relativism of the Dewey Lectures (on their interpretation) is more akin to the practical and political interpretation than to *The Theory of Justice*. They seem to think the opposite; but where explanation is needed, there is only muddle—a cryptic notice that it is "a precondition of debate" that the debaters "each view themselves at some level as having the weight of reason on their side" (p. 150). That Kukathas and Pettit present only a caricature of Rawls's political liberalism will not surprise anyone who has suffered through the extraordinary floundering of the final chapter of this work. Having announced that Rawls went from a Kantian to a Hegelian phase, the authors simply fail coherently to explain why he did so, why the two principles have remained much the same throughout, why Rawls should continue to use the label *Kantian constructivism*, and so on. Hampered by a misleading interpretive scheme and a feeble grasp of Kantian constructivism, they splash around terms like *Kantianism*, *Hegelianism*, and *pragmatism* with dizzying imprecision, substituting italicization for explanation. The very nature of Kantian constructivism is left a mystery. Rawls's post-*Theory* approach is assimilated to everything from Hobbesian peace seeking to Rorty's pragmatism; but the basic contrast with rational intuitionism is not discussed at all!

If Rawls were simply concerned with underwriting U.S. doctrinal conformity (a job that hardly merits a night watchman), his theory would certainly be in very desperate need of emboldening. However, there is a potential radicalism in Rawls's attempt to use the ideal of the free and equal citizen, at home in such areas as freedom of conscience, to sort out problems of distributive justice (see J. Cohen, "Democratic Equality," *Ethics* 99 [1989]). Kukathas and Pettit consistently ignore or distort this side of justice as fairness. Too many crucial issues (Kantian constructivism, antirealism, etc.) were never on their agenda.

University of Chicago BART SCHULTZ

Gender, Identity, and the Production of Meaning. By Tamsin E. Lorraine. Boulder: Westview, 1990. 227p. $38.50 cloth, $14.95 paper.

Feminism without Illusions: A Critique of Individualism. By Elizabeth Fox-Genovese. Chapel Hill: University of North Carolina Press, 1991. 347p. $24.95.

Feminist Interpretations and Political Theory. Edited by Mary Lyndon Shanley and Carole Pateman. University Park, Pennsylvania State University Press, 1991. 288p. $35.00 cloth, $12.95 paper.

Of the three books under review here, the volume edited by Shanley and Pateman is the most accessible for the nonspecialist. It draws together a variety of excellent papers (many previously published in disparate places),

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each of which shows how new light can be shed on the major classical and contemporary texts of political theory by asking questions about women, gender, sexuality, and the family and how these intersect with such perennial issues as the nature of citizenship, the public and private realms, equality and justice. The book is arranged chronologically by theorist, beginning with Plato and Aristotle and ending with Rawls, Foucault, Arendt, and Habermas. Sandwiched between are essays on Hobbes, Locke, Rousseau, Hegel, Marx, and J. S. Mill, as well as Mary Wollstonecraft and Simone de Beauvoir. All the essays are clearly written, insightful, and of interest to generalist and specialist alike. One regrettable gap, however, is the absence of a treatment of women in the Christian tradition. An examination of ecclesiastical misogyny and its place in both early Christian and medieval thought would have more fully completed the volume as a feminist companion to the teaching of the entire historical canon.

The inclusion of essays on Wollstonecraft and Beauvoir is rightly defended by the editors on the grounds that they are the authors of important works in the history of political thought and ought to be considered part of the canon (p. 2). The case for Wollstonecraft's inclusion is made well by Moira Gatens, who explores the ambiguities that the liberal paradigm presents for women through a reading of Wollstonecraft's texts. However, Beauvoir is less well served by Elizabeth V. Spelman's treatment. Although Spelman's critique of Beauvoir for failing to acknowledge the significance of differences between women is important and skillfully argued, it does not function well in this collection as a means of integrating Beauvoir into the canon. The editors could, in my view, also have been a little bolder in integrating more women writers into the canon. For example, an essay on Olympe de Gouges's intriguing Declaration of the Rights of Woman or a treatment of Harriet Taylor Mill would not have been out of place here. But such minor criticisms aside, this book usefully puts together a set of papers that are both accessible and of uniformly high quality.

Tamsin Lorraine's *Gender, Identity, and the Production of Meaning* also bears in potentially important ways on the reading of political theory texts, although this is not her primary focus. Lorraine's concern is with the place of gender-differentiation in the formation of the self, particularly of the authorial self. Rather intriguingly, she draws on both Lacanian and object-relations revisions of Freud to develop a psychoanalytic account of a subject, or self, who emerges from social codes and constraints but only through what she calls *self-constituting activity*, or *self-strategies* that also involve intentionality and choice. The masculine self, in its pure form (to which, of course, no empirical individual ever wholly conforms), corresponds to full initiation into Lacan's symbolic order. The pure masculine is "changeless, eternal, harmonic order . . . the real rendered orderly" (p. 184) and masculine self-strategies involve constituting the self by clear separation from, and opposition to, the other. By contrast, "the purely and eternally feminine is chaotic flux" (p. 183); and the feminine strategy for self-constitution is one of connectedness, even fusion, with the other.

Too much of Lorraine's book is taken up with giving an extensive summary of the psychoanalytic literature on which she draws. The book becomes most interesting in its penultimate section. Here, she takes up the question of reading texts through the gendered self-strategies of their authors. In insightful readings of works by Kierkegaard, Nietzsche, and Sartre, she shows how gaps, incoherences, and inconsistencies in philosophic texts can be read as symptoms occasioned by the inability of their authors to renounce the male self-strategies to which, with varying degrees of intensity, they hold.

Lorraine's psychoanalytic accounts of self-constitution undoubtedly help to explicate the works of individual authors, but they cannot form the basis for an adequate account of selves in social and political life. In her conclusion, Lorraine tries to build an argument for feminist politics from her account of gendered self-strategies, but she ends up in rather naive exhortation. We must, she asserts, develop a "new self" incorporating both feminine and masculine self-strategies (opposition and connectedness). Moreover, such a new self could also be the basis for a "new world view"—for a "world community," even. But how such new selves are to come into existence in a world of already-gendered power relations is not discussed. Nor are the difficulties endemic in modeling whole societies (let alone a world community) on the dyadic psychoanalytic relation of self and other even acknowledged.

If Lorraine is insufficiently sensitive to the distinctions between selves and social wholes, the same cannot be said of Fox-Genovese. Indeed, the central theme of *Feminism without Illusions* is the tension between individualism and the ideal of community as it plays out today both in feminist theory and more generally in U.S. political and cultural life. In essays that range in their specific topics from concrete political issues (e.g., affirmative action, abortion, and pornography) to treatments of the impact of postmodernism on the history profession today or the debate about the canon and cultural diversity, Fox-Genovese's leitmotif remains the fraught nature of the relation between individualism and community. While some freedoms have indeed been won for all in the name of individualism, individualism's construction of the self as male and white has made it of ambiguous value to women, African Americans and other minorities. The way forward, she argues, must be to abandon a conception of atomistic, innate, individual right for "a view of individual right as derivative from collective life" (p. 244).

Fox-Genovese writes engagingly, with an ease and elegance born of her tremendous erudition about U.S. history and culture. She is at her best in pointing out the contradictions that bedevil U.S. feminism because of its failure consistently to escape from individualism. For example, she rightly points out, while feminists make much of women's "different voice" and their orientation to community and caring, they have defended access to abortion on wholly different (and, indeed, incompatible) grounds—as an issue of individual rights, grounded in a crudely possessive individualist conception of woman as the owner of her body.

Although critiques such as this are thought-provoking, there are also some serious difficulties with the book, especially if one asks what its positive implications might be. The two central concepts, individualism and community, are not explicated carefully enough. At first, individualism means "possessive individualism"—an atomistic individualism, which has grounded liberalism by asserting innate, personal rights as claims against society and the state. But as the book progresses other versions of individualism appear on the scene.

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Epistemological positions that stress "difference" are a form of individualism. So, at heart, is postmodernism (surely news to its adherents!). Even conservatism (historically the defender of community against naked egotism) is today marked by individualism. Are all these forms of individualism essentially one and the same? Are they all equally problematic for women? I suspect not; but Fox-Genovese does not tell us.

Even more perplexing is the status of community for Fox-Genovese. Traditional theorists of community (such as Tönnies, whom she mentions repeatedly) stress its ascriptive qualities. Yet ascriptive communities have, she concedes, been highly oppressive of women. But if ascriptive communities cannot function today as mitigators of individualism, neither can voluntary communities; for, she alleges, conceptions of community based on a perceived common identity or commitment continue unacceptably "to ground community in individualism" (p. 40).

What Fox-Genovese finally points to as a possible basis for community is the existence of a U.S. national culture, grounded in the canon. Although she acknowledges that the canon needs some expanding to include previously excluded groups, she believes that it does fundamentally define a national community. But even if we accept this controversial claim, such a notion of cultural community can hardly provide an adequate basis on which to ground her political appeal that we should abandon individualist conceptions of rights for a view of individual rights as derived from collective life. In short, the book raises more questions than it answers—but at least they are serious questions.

Oberlin College SONIA KRUKS

The Meaning of Socialism. By Michael Luntley. LaSalle, IL: Open Court, 1990. 214p. $32.95 cloth, $14.95 paper.

The Concept of Socialist Law. By Christine Sypnowich. Oxford: Clarendon, 1990. 195p. $45.00.

The events since 1989 have thrown well-deserved and long-overdue doubt upon both the viability and moral defensibility of actually existing socialism. But few would argue that even the collapse of the Soviet system and its satellites has mortally damaged the idea of socialism even if the term itself is widely held in disapprobrium. When the Berlin Wall crumbled, East German citizens were found to want both the security of socialism and the affluence of the market, not merely "freedom." Scandinavian or West European–style social democracy—not U.S. or Chilean capitalism—remains the preferred ideal to many formerly socialist lands. The principles of socialism thus retain a considerable claim on the attention of modern political theorists. Michael Luntley's aim is accordingly not to vindicate specific socialist governments but to offer a philosophical justification for interfering with individual choices for the overall benefit of society. Socialism is here, therefore, a higher form of morality and, particularly, a vision of the good life that is independent of individual preferences at any given time. Luntley's chief targets in this cogent, persuasive, and admirably lucid book are those liberals, (of late, most notably, Rawls and Nozick) to whom the good can only be defined in terms of individual preferences (a view that might be termed "market positivism"). Instead, he asserts that "primary goods," such as education, exist and should be made codeterminate with citizenship. These goods, in turn, are defined and protected by communities and non-market-oriented groups, whose aims and activities are usually eroded by an overemphasis on market freedom. But Luntley also contends that socialists have stressed a number of issues (e.g., class, equality, justice, and rights) to the exclusion of a concern with community. He aims to please no particular constituency. Socialists will be troubled by his reluctance to concede public ownership as central to their vision, and puzzled by his view that small businesses might accord more fully with socialist aims. (But they can learn much from this critique—so appropriately the product of the rule of the shopkeeper's daughter from Grantham.) Liberals will probably not accept his assault on entitlement theories and, with their opponents, will doubt whether Luntley's claim of the good life divorced from public opinion and market research can be substantiated. Yet Luntley is absolutely right to postulate the problem of non-market-guided ideals or goods as central to the question of what the public interest is. A clean environment and limitation on population growth, for example (which some would argue now take precedence even over the issue of social justice), are two such elements of the good life that the market cannot engender and that demand political action whether the majority can be persuaded of its relevance, timeliness and cost or not. Luntley's defense of interference is thus, as a restatement of the old juxtaposition of negative to positive liberty, relevant to any debate about the ends of society; and his is correspondingly an extremely useful starting point for discussions about first principles.

For all its sense of embodying a higher form of morality, socialism (especially Marxism) nonetheless has never been entirely sure whether its own universalistic claims did not supersede all previously class-bound morality and whether, in turn, the struggle to usher in the new social order did not justify measures in the name of the largest class that most previous moral theories condemned as repugnant. Luntley denies that assessing the components of the good society means forcing any to be free. But how are minority rights to be protected, and under what criteria of general good ought they to be suppressed? This has been the problem of socialist law: if all systems of law are class-derived—indeed, if law itself is a function of class society doomed to disappear with classlessness—what is the status of a proletarian legal system once the new order is achieved, and what should its relation be to questions of *bourgeois justice*, such as the death penalty? Christine Sypnowich recognizes that most socialists, for reasons linked to antinomianism, class analysis, and conceptions of primitive communalism and higher forms of fraternity, have dismissed bourgeois justice out of hand. With this has been ejected much of the language of noneconomic rights, the notion of a rule of law removed from the context of class dictatorship, the separation of executive from judiciary, and so on. Following the lead of Tom Campbell (*The Left and Rights 1983*) and others, Sypnowich contends that this dismissal of the centrality of law to modern society ignores the fact that the substantive justice sought by socialists cannot be achieved without a framework of legal institutions to arbitrate between public and private claims. Instead of "withering away," law would thus sprout new forms in response to new

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socialist theories of coercion, justice, and class power. (Sypnowich simply assumes that the new society has abolished private ownership of the means of production.)

After reviewing the sources and components of the Marxian conception of the supersession of law and Soviet debates over aspects of the theory, Sypnowich examines current debates among natural law theorists, legal positivists, and others about the elements of legal conduct and their relation to morality. She also gives a good summary of the evolution of Soviet legal practice and then concentrates on four troublesome areas: (1) freedom and the rule of law (where the threat posed by capitalism to the rule of law is central), (2) human rights and political reform (arguing for a socialist conception of rights based on the idea of human dignity), (3) rights in relation to altruism, and (4) the obligation to obey socialist law. This is much the best account available of the *possibility* of a socialist legal system vastly different from that usually associated with the term. Like Michael Luntley, Sypnowich anticipates an era of political and legal reform in which much old dogma will be abandoned but new ways of establishing a higher form of social justice still informed by classical socialist theory will be attempted. Sypnowich's book might have benefited from a starting point accepting pluralistic property relations as an integral part of socialism, like Luntley's; for property disputes are at the core of every legal system and would be under her own version of superior socialist legality, and all forms of future socialism are bound to be considerably more diverse in this respect than existing socialism ever was. Nonetheless, both of these authors help us to recognize that the debate about socialism, while it may assume many new forms, is hardly yet laid to rest.

University of London GREGORY CLAEYS

Private Property and the Limits of American Constitutionalism: The Madisonian Framework and Its Legacy. By Jennifer Nedelsky. Chicago: University of Chicago Press, 1991. 343p. $29.95.

Property and the Politics of Entitlement. By John Brigham. Philadelphia: Temple University Press, 1990. 223p. $39.95.

These are two books about the *new property*—a concept of property that goes well beyond the traditional view (as reflected in constitutional provisions) to embrace sources of government largess or entitlements. Both authors support this broadening of the concept of property to include procedural protections to propertylike transfers. Jennifer Nedelsky argues that private property has shaped the structure of the U.S. political system and the continuing tension between popular democratic government and the security of property. She claims that the constitutional focus on property has limited the strengths of the U.S. political system and has provided the source of its major weaknesses. These weaknesses lie in the failure of the system to realize its democratic potential by unequally allocating political power and wealth. This inequality was enshrined within the Constitution in an effort by property holders to limit the ability of the majority to use the power of the state to redistribute. Nedelsky claims, however, that the concern to protect private property rights distorted both legal and economic institutions and laid the framework for the current conflict between the traditional rights of property and equality within the modern welfare state. She believes that there is a need to redefine the scope and purpose of the state to accomplish democratic objectives.

To develop the arguments in her book, Jennifer Nedelsky focuses on the arguments of three critical framers of the Constitution: James Madison, Gouverneur Morris, and James Wilson. These people, she argues, encompassed the spectrum of the views on property and democratic participation at the constitutional convention. This discussion covers chapters 1–5, and chapter 6 develops some of the legacies and conflicts that Nedelsky believes resulted from constitutional protections of private property rights.

John Brigham also focuses on the legacies of constitutional guarantees of private property although he is less concerned about the origins of these practices and is more concerned about the debate over redistribution. He calls for an extension of the concept of property to include entitlements, which he defines as transfers that individuals receive, and form expectations over, as rightfully and legally theirs.

To develop his case for viewing statutory entitlements as property, Brigham examines various case studies of land policy, especially zoning issues and the protection of the California coast and tax breaks for environmental uses of the land. These are enlightened actions, he believes, that reflect a more activist view of property. He also points to social security benefits as examples of entitlements that should have propertylike status.

Both Nedelsky and Brigham seek to democratize the notion of property, and it might be that the inclusion of transfers as "property" would lead to redistribution of wealth; but it is not obvious that a more equal distribution would result. The data I am aware of suggest that most transfers go to the middle class and the wealthy, not the poor. Do Nedelsky and Brigham want to enshrine as property the transfer payments received by the nation's peanut, sugarcane, and sugarbeet growers; corporate holders of federally subsidized water; ranchers who pay below-market grazing fees; or middle-class parents whose children attend state universities and pay tuition that covers only a fraction of the costs of their education? These transfers surely fit the criteria outlined by Brigham as propertylike entitlements. I doubt the poor would benefit from attaching more formal property rights to these payments; nor do I believe that these are untypical of the direction of transfer flows. It is also unclear whether constitutional protections of property are really the basis for the unequal distribution of wealth in the society. The little information that we have about wealth distribution in the Soviet Union and Eastern Europe suggests that substantial inequality has existed in those societies with major transfers to party officials and senior bureaucrats even though private property by and large did not exist. Wealth distributions are even more skewed in Third World countries where the protections of property rights vary enormously.

These points illustrate what is so unsatisfying about both books. Neither employs standard social science techniques, in which verifiable hypotheses are presented and evaluated systematically against available evidence. Although Nedelsky examines the positions of three major participants in constitutional debates regarding property protections, the link to current institu-

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tions (or "distortions" of them) is not credibly made. Nowhere do Brigham or Nedelsky blend their work with the existing (and large) literature in economics and political science on the path and sources of wealth inequality. This would have been useful in developing their arguments regarding the link between property definitions and wealth distribution.

Even more disturbing is their failure to examine the implications for the level of wealth in the society of a redefinition of property rights. Within economics and political science there exists both a theoretical and empirical literature regarding the definition of property rights and incentives for economic activity, with which these authors show no acquaintance, much less appreciation. Nevertheless, research indicates that the incentives to engage in socially beneficial investments and other economic activities with delayed returns depend crucially upon the security of property rights. Sustained economic growth is rare both historically and currently. The failure of economic development to spread to most of the world's population is at least in part due to insecure or unstable conditions and the incentives such an environment provides for short-term and wasteful economic behavior. The principal source of this insecurity lies in weak property institutions and the arbitrary and frequent confiscation of property by the state at the behest of powerful groups. The broad redefinition of property rights to include entitlements outlined in the two books would remove restrictions on the power of the government to reassign rights in response to the emergence of new and influential interest groups in the United States. Since there is a dynamic process of interest group formation and influence, one can anticipate that over time new and different entitlements or demands for "property" would arise.

Following Brigham and Nedelsky, a democratic government should respond. This is not, however, the environment that will encourage long-term investment or other activities that bring advances in economic welfare. A redefinition of property rights might lead to a reallocation of wealth, though current transfers do not make this obvious. On the other hand, however, it seems quite likely that the new definition of property they call for would lead to a smaller level of wealth to reassigned. Lower levels of economic activity are unlikely to benefit the poor—presumably the people the modern welfare state is supposed to help.

University of Arizona GARY D. LIBECAP

Finding the Mean: Theory and Practice in Aristotelian Political Philosophy. By Stephen G. Salkever. Princeton: Princeton University Press, 1990. 287p. $35.00.

Aristotle on the Human Good. By Richard Kraut. Princeton: Princeton University Press, 1989, 379p. $37.50.

A major development within contemporary practical philosophy, parallel to the Kantian revival, has been the resurgence of interest in Aristotle. (While on balance disapproving, Judith Shklar has called him "the man of the hour.") The recovery of his thought has proceeded unevenly. Interest in his ethical works has been enormous (witness the lush profusion of competing interpretations) and has contributed to the revival of virtue ethics over against rule morality.

By contrast, the reappropriation of Aristotle's politics is less advanced. It is not hard to see why. His treatment of women and slaves seems at odds with modern egalitarianism; the priority he accords to politics seems hostile to both liberal privacy and liberal individualism; his understanding of political agency seems to rest on discredited metaphysical and biological premises.

The uneven development of contemporary Aristotelianism is affected, as well, by the academic division of labor. Departments of classics and philosophy have focused on the ethical works, both as centerpieces of the densely contested arena of classical scholarship and as counterpoints to Kantian and utilitarian moral theory. The *Politics* and *Rhetoric* have been largely consigned to political science departments, where until fairly recently they have received far less attention.

The books under review, each excellent and instructive in its own way, reflect this state of affairs. Richard Kraut is able to plunge in medias res, unapologetically offering his interpretation of Aristotle's conception of happiness and defending it against well-developed rival views. By contrast, Stephen Salkever is compelled to spend a great deal of energy on threshold questions, especially the question of the relevance and acceptability of Aristotle for contemporary political inquiry.

Kraut is quietly bold. Against prevailing inclusivist interpretations, he argues that Aristotle identifies happiness with a single rational activity—philosophical contemplation. The concluding sections of the *Nicomachean Ethics*, contrasting and rank-ordering politics and philosophy, do not contradict but, rather, complete, the discussions of human happiness and function with which the work begins. Kraut's Aristotle is quasi-Platonic (even Socratic), concerned with the conflict between philosophy and politics and determined to vindicate the claims of theoretical, over practical, reason. His Aristotle defends philosophy as requiring less "equipment" than does politics. Devoting one's life to contemplation diminishes life's vulnerability to fortune; the goodness of philosophy is not as fragile as fortune's.

This defense of theoretical reason does not, on Kraut's reading, undermine moral virtue. Philosophical activity is compatible with—indeed, requires—the practice of the moral virtues as understood within the practical realm. (This is the case in part because Aristotle subtly "intellectualizes" these virtues.) Moreover, Kraut argues at considerable length that Aristotle's ethical agent is not an egoist. Philosophers can, on occasion, rationally devote themselves to the well-being of family, friends, and fellow citizens at the expense of their own good.

Kraut's book is distinguished by its impeccable scholarship, careful argumentation, and respectful engagement with rival interpretations offered by Ackrill, Cooper, Hardy, Irwin, and Nussbaum (among others). While it will not resolve the debate over Aristotle's conception of *eudaimonia*, it moves that debate forward in ways that cannot be ignored. My one regret is that the index to this otherwise beautifully produced book is not detailed enough to be fully serviceable.

Salkever defends Aristotle's political philosophy as a distinctive middle way between foundationalism and conventionalism and between theories of mechanistic determinism and unrestricted freedom. His Aristotle views practical philosophy as the vehicle not for resolving political disputes but, rather, for clarifying the terms in which they must be conducted. At the heart of sound politics is well-developed practical reason, expressed in

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joint deliberation sensitive to context and particularity. This conception of practical reason reflects an understanding of human biological specificity that is not (despite MacIntyre) metaphysical and hierarchical but, rather, empirical and inclusive. In its antiprecisionism, it emphasizes virtues over rules; in its focus on open deliberation, it is at least compatible with a deepened understanding of modern democracy.

While Salkever agrees in general terms with Kraut that Aristotle ultimately gives pride of place to philosophy over politics, he denies that this rank-ordering (or, for that matter, any other theoretical proposition) has direct consequences for political practice. The central practical function of theory is negative—to teach political actors that the mechanical application of general rules is bound to be inapt. Salkever's discussion of this thesis is subtle and provocative. Still, one wonders whether his interpretation does not overemphasize sensitivity to particulars at the expense of more general considerations, a worry heightened by his relative neglect of Aristotle's moderately precisionist discussion of distributive justice.

Salkever's book is not intended—and will not serve—as a comprehensive guide to Aristotle's political philosophy. It is, rather, a highly intelligent effort to situate that philosophy within contemporary debates in practical philosophy and social science and to argue that social inquiry carried out in a broadly Aristotelian spirit can reach insights not otherwise available. Taken as a plea to political scientists and supporters of liberal democracy to take Aristotle seriously, this book deserves respectful consideration.

University of Maryland, College Park WILLIAM A. GALSTON

The Genesis of the State. By Martin Sicker. New York: Praeger, 1990. 161p. $39.95.

The Changing Architecture of Politics: Structure, Agency, and the Future of the State. By Philip G. Cerny. London: Sage, 1990. 267p. $47.50 cloth, $18.95 paper.

Martin Sicker is a political scientist and also a practitioner (having served as a "senior executive in the U.S. government.") But *The Genesis of the State* has about it an air of the abstract and definitional. He poses questions that positively guarantee theoretical overreach, for example: "How did the state originate and what is the source of its authority?" (p. viii). Many of the greatest minds in political thought and historical study have taxed themselves to offer answers (or at least useful suggestions) in this matter. Sicker dispenses with the question of origins in 24 short pages. This is bound to be unsatisfactory, in large part because he has aimed too high and come up with too little.

Untouched by some of the more interesting debates in recent political science scholarship of a feminist and Marxist stripe, Sicker forges forth, proliferating definitions and relating texts to other texts in this expository work. But he seems not to appreciate the great and abiding fears and hopes that animated both state theorizers and state builders historically. "Adequate definitions" get us nowhere if the place from which we begin has no visible means of support. Simply to assert (as Sicker does) that "the state is the corporate structure, coextensive with a political society, which is the locus of supreme political authority, and which can command an effective force monopoly to assure compliance with its decisions" (p. 9) is to insulate through definitional flat what, in fact, needs to be explained and interpreted. One completes this book, with its quick once-overs of the *divine, patriarchal, force, organic,* and *consent* Theories, without any new illumination on how states emerged, how the state form got universalized, or why men and women remain prepared to sacrifice and to die in the name of the state. Any theorizing about the state ought to help us (minimally) to get our bearings with such mordant matters.

Philip Cerny's *Changing Architecture of Politics* is a conceptually "thicker" project—hence more provocative and ultimately helpful. The prose, alas, is terribly clotted. No doubt, devotion to the concept of "structuration" automatically means that words will not flow liltingly from the word processor. Cerny is devoted to the notion there is some point of origination to be located or some core to the problem to be adumbrated. He finds the core of the problem in asking "whether institutions and structures develop and operate according to an independent dynamic, and, if so, how" (p. x). If one does not share his conviction, the book will probably not be particularly compelling reading. Cerny opts for a "structural or institutional theory of politics." Unlike Sicker, he has reviewed most of the relevant literature, although feminist theorizing is once again conspicuous by its absence. He tries to weave his way in between overly idealized and overly materialistic theories of politics and the state. And he is attuned to the puzzlement of how it came about that "an essentially contingent and polycausal generation process can result in a relatively coherent, homogeneous, and structurally durable result" (p. 27). *Durable result* bears an unfortunate association with things like reliable tires, but one takes the point.

Cerny's insistence that the state is *not* "one, monolithic, state apparatus, but several levels and layers of structured games" (p. 47) is an attunement to complexity that Sicker misses. But finally, this book, too, disappoints. It omits the constitutive role of ideas—absolutely essential to any consideration of the creation of the early modern state and the concept, and practice, of sovereignty. Finally, despite Cerny's bow in the direction of "agency and choice," we are left with little insight into how states become the repositories of human hopes and help to structure our very identities. Two books that serve as a welcome antidote to the weaknesses of the volumes here reviewed are Benedict Anderson's *Imagined Community* and Eugene Weber's *Peasants into Frenchmen*.

Vanderbilt University JEAN BETHKE ELSHTAIN

A Dream Deferred: America's Discontent and the Search for a New Democratic Ideal. By Philip Slater. Boston: Beacon Press, 1991. 224p. $19.95.

Democracy and Equality: Theories and Programs for the Modern World. By Ronald M. Glassman. New York: Praeger, 1989. 234p. $42.95.

Both of these thoughtful and readable books are very much concerned about the future of democracy and the need to think about, and act upon, the problems that

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face democracy in the United States. Slater, a wide-ranging social critic, asks, How can the United States move more genuinely beyond the era of authoritarianism to a fuller democratic megaculture? His thesis is that the democratic megaculture requires the complete rout of authoritarianism in its many forms—military, political, economic, social, religious, educational, medical, and psychological. Glassman, a sociologist keenly concerned about social policy, asks, "How can we institutionalize the ethic of social justice alongside competitive individualism without destroying the liberal structure of legal-democracy and market economics?" (p. 204). He advances the thesis that the achievement of democracy and equality requires us to build on, but find correctives for, liberal democracy. He argues that political democracy is essential and a market economy has much to commend it, but the sensible pursuit of equality calls for a modified capitalism drawing upon the corrective insights of Aristotle, Rawls, and Keynes. Taking into account valid objections to equality to be found in Tocqueville, Weber, Hayek, and the neoconservatives, Glassman would retain the strengths of capitalism and the market economy while avoiding the weaknesses of capitalism and classical liberalism. His model, which rejects the utopian vision of both socialism and classical liberalism, is a variety of liberal, capitalistic democracy.

Slater's attack on authoritarianism (and its four pillars: institutionalized submissiveness, systematic oppression, secrecy, and deflected hate) is hard-hitting and cogent, even when sometimes exaggerated or overstated. His defense of democracy as efficient, flexible, innovative, and participatory is heartening (if sometimes open to challenge). His argument on behalf of decentralization is only partly convincing, largely because of his failure to recognize fully enough that some centralization of power is vital in a democracy and that some varieties of decentralization can be antidemocratic and often grossly inefficient. His argument against authoritarianism would have been strengthened by a clearer recognition of the case for legitimate democratic authority. And is Slater correct in his view that the United States is out of tune with the worldwide democratic trend and that U.S. citizens complacently believe that they have already made the transition to democracy?

Many feminists will applaud Slater's contention that traditionally female traits like "flexibility, creativity, sensitivity, understanding, emotional honesty, directness, warmth, realism, and the ability to mediate, to communicate, to negotiate, to integrate, to cooperate" are "precisely those most needed to successfully maintain a democratic society" (pp. 40–41). And they may agree that "since women are far less committed to authoritarianism than men are—less beholden to it, more oppressed by it—they are perhaps in the best position to assert the values of democracy, the values of cooperation, integration, negotiation, openness, and equality" (p. 95). All democrats will cheer his affirmation that "no real democratic tradition can take root in a society in which the macho tradition holds sway" (p. 41)—even if some may have reservations about his contention that "sexism is the greatest obstacle to the achievment of full democracy in any society" (p. 85).

Both Slater and Glassman reveal the modern intellectual's emancipation from communist utopian dreams. Both are highly critical of the authoritarianism of communism's huge centralized bureaucracy. Both recognize capitalism's freedom, initiative, and creativity but fault its authoritarianism and its disregard of equality. Slater's contention that "democracy cannot survive inequality" (p. 68) is a major motif of Glassman's book. Slater stresses the importance of achieving democracy in the workplace; Glassman stresses the need to modify capitalism so as to ensure decent jobs and to overcome distressing inequalities of wealth and class. Glassman is, however, more concerned than Slater with maintaining the vitality of the middle class.

Glassman's argument is enriched by a stimulating effort (chaps. 1–3) to find correctives for liberal democracy and capitalism in Aristotle, Rawls, and Keynes. He endorses Aristotle's efforts to build his constitutional democracy on the foundation of a solid middle class. He shares Rawls's efforts to modify the Lockean contract and employ the difference principle to do justice to the disadvantaged. He admires Keynes's efforts to make capitalism stable and society more equal. Slater's emphasis, by way of comparison, is on the need for a more radical participatory democracy. Of the two, Glassman seems more in touch with social policy and realistic possibilities for change than does Slater. In many respects, however, Slater's critique of authoritarianism (and especially of sexism) cuts deeper than Glassman's critique of liberal democracy and capitalism.

Both authors are clearly critical of the status quo and seek to get beyond it. Slater's critique is the more far-ranging of the two—embracing as it does a critique of religion, education, the medical establishment, and (the wrong kind of) individualism. Glassman's critique, although committed to the advance of equality, is more respectful of valid objections to theories of equality (chaps. 4–5) and more eager to meet those valid objections in practical programs of action (chaps. 6–7).

Slater is better at telling us what is wrong with the United States today than in informing us what to do to overcome such major obstacles as the widening gap between rich and poor, the persistence of military attitudes, the tenacity of authoritarians in power, and public apathy. His efforts to prepare the ground for greater democracy include demilitarizing, containing greed, providing a future, promoting sexual equality, freeing our imaginations, opening communication, and freeing ourselves" (pp. 186–88). Although laudable, these rubrics are, alas, not all that helpful to practical reformers. Glassman, who sometimes advocates federal programs and assistance for the poor or needy that might contradict his opposition to centralization and inefficient bureaucracy, poses tough choices facing the practical reformer.

Even though the critiques of Slater and Glassman are sometimes overly ambitious, we are nonetheless seriously challenged by these two provocative authors and their fundamental concern for the future of democracy.

Drew University NEAL RIEMER

Friedrich Nietzsche and the Politics of the Soul: A Study of Heroic Individualism. By Leslie Paul Thiele. Princeton: Princeton University Press, 1990. 235p. $35.00 cloth, $9.95 paper.

According to Leslie Paul Thiele, the kernel of Nietzsche's thought involves a highly personal exploration of what it means to become a sovereign individual. Thiele argues convincingly that the conceptual content

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of Nietzsche's philosophy cannot be separated from its autobiographical origins without loss; his ideas are meant to be understood in terms of the existential pathos that produced them—a pathos of perennial striving to become "what one is."

Nietzsche's choice of a political vocabulary to describe his inner strivings is, in Thiele's view, no accident. For Nietzsche, the human soul is at bottom not a unity but a multifarious chaos of conflicting drives characterized by endless struggle. The multiple voices and perspectives of Nietzsche's writings are a reflection of the tensions of his multiple soul. According to Thiele, "Politics . . . conveys most of the meanings inherent in the development of individuality: the struggle, the ambiguity and ambivalence, the will to power, the compromises and coalitions, domination and rule, plurality and rank, the search for organic unity" (pp. 3–4). In those who have the strength for it, a semblance of unity is achieved through struggle, domination, and conquest on a subliminal level. The "higher man" is he who emerges with an aristocratically ordered soul entailing a hierarchically ordered agonal community of instincts.

Thiele is careful to point out that Nietzsche eschews the egalitarian implications of liberal and democratic individualism in favor of a heroic individualism that is inaccessible to the majority. The Nietzschean hero is one who can affirm life as an ongoing process of becoming through struggle and self-conquest. Nietzschean *amor fati* (love of fate), does not imply a passive fatalism but, rather, the total affirmation of one's involvement in a world in which one is fated to do battle with *fortuna*. The term *hero* may seem problematic, but Thiele offers qualifications. He notes, for example, that Nietzsche repudiates "the cult of the hero" as just another form of idolatry. Thiele also suggests that at the highest level (perhaps unattainable) the perfectly ordered soul of the overman would not need to engage in heroic struggle.

The core of the discussion is not structured around Nietzsche's "doctrines" but, rather, around what Thiele takes to be the various incarnations of the Nietzschean ideal: the philosopher, the artist, the saint, and the educator, and the solitary. Thiele has produced a clear, gracefully written book that offers an insightful and, on most levels, extremely satisfying interpretation. He avoids the pitfalls of turning Nietzsche into the last metaphysician, a deconstructionist, a pragamatist, a literary stylist merely, or a Nazi. He does justice to Nietzsche the seeker of knowledge, as well as to Nietzsche the radical skeptic. And his account of how the chaos of the soul orders itself is more convincing than either Walter Kaufmann's or Mark Warren's because of its greater fidelity to Nietzsche's own remarks on the instincts, reason, and life as will to power.

I only wish Thiele had taken more seriously the social implications of the ideal and the view of life that his subject propounds. Unfortunately, he never discusses the many troubling passages about politics and society in Nietzsche's writings except summarily to discount their significance with arguments reminiscent of Kaufmann's. But real politics do appear to matter to Nietzsche—and understandably. His goal is not only his own self-perfection but, more generally, "the enhancement of man." Again and again, Nietzsche professes a commitment to the promotion of a higher human type: "Every enhancement of the type 'man' has so far been the work of an aristocratic society—*and it will be so again and again*—a society that believes in the long ladder of an order of rank . . . and that needs slavery in some sense or other" (*Beyond Good and Evil* 257; my emphasis). Politics matter even though the Nietzschean philosopher is not a political actor, because his highest efflorescence is facilitated by favorable social and political conditions.

Nietzsche's view of tension and struggle as life-enhancing has social and political implications that parallel the psychological ones: "The highest type of free man should be sought where the highest resistance is constantly overcome: five steps from tyranny. . . . This is true psychologically if by 'tyrants' are meant inexorable and fearful instincts that provoke the maximum of authority and discipline against themselves. . . . This is true politically too; one need only go through history" (*Twilight of the Idols*, sec. 38). It is not only because spiritual struggle resembles political struggle that Nietzsche uses political language to describe the soul. More importantly, it is because there are analogies between the well-constituted soul and the well-constituted society, with the soul of the highest type closely resembling the kind of tension-filled aristocratic order Nietzsche both admires and desires.

Criticisms aside, Thiele's has written a book of very high quality that makes an important contribution. His account of the politics of the soul is excellent; and there can be little doubt that the politics of the soul is of far greater interest to Nietzsche than the politics of the world.

Florida International University BRUCE DETWILER

The Forms of Power: From Domination to Transformation. By Thomas E. Wartenberg. Philadelphia: Temple University Press, 1990. 253p. $34.95.

What happens when philosophy meets political science? It is a loaded question, to be sure, but one that characterizes Thomas Wartenberg's very careful and thoughtful examination of the metaquestion of *power*. Wartenberg distinctively uses prominent philosophers to make his points about particular questions in the analytical debates about power. He simultaneously articulates his own approach to power in a way that seeks to circumvent the conceptual traps that have stymied political theorists. In the process, the analytical discourse on power is placed in a broader context while its narrative is enriched.

Wartenberg's analysis is premised on the insight that social power takes a variety of forms, has different uses, and therefore cannot meaningfully be integrated into one all-encompassing theory. He rejects the idea that power is an essentially contested concept like art or democracy, arguing, instead, that competing conceptions of power conceptualize different, if related, phenomena. For instance, he accepts the distinction between *power-to* as ability versus *power-over* as constraint; however, he sees them not so much competing with each other as conceptualizing different things. He focuses on power-over because of its crucial implications for criticizing social inequities.

Wartenberg shows how power-over can be positive, as well as negative, as in the case of paternalism. He shows how power-over involves both consensual and coercive aspects. Power-over is articulated through different types of power—force, coercion, and influence—

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and often simultaneously. These are quite distinct from its uses ranging from domination to transformation. The former involves the exercise of power at the expense of the subordinated while the latter promotes their empowerment. Wartenberg adds that the interpersonal dyad in a power-over relationship is structurally situated. Theories of power-over must also be dynamic and account for the temporality of power. Power-over relationships involve anticipations of future actions that might be undertaken by primary and collateral parties.

These analytical investigations create the basis for Wartenberg's own theory of power-over—aptly titled a "field theory of power." It is, of course, a structural, dynamic theory of how power-over can be used for both domination and transformation. While heavily dependent on a select group of theorists ranging from Plato to Sara Ruddick, Wartenberg manages to offer his own distinctive contribution. He stresses that power-over is not reducible to a behavioral trait or individual possession. He uses Hegel, Marx, Nietzsche, Giddens, and Foucault to show how power operates in a situated, structural context rigged from above or below. He does so without subjectifying power, as he suggests Foucault does. Instead, he maintains a place for human agency while underscoring that individuals' "action-environment" as well as their "action-alternatives" (and therefore the individuals themselves) are structurally constituted.

Wartenberg's articulation of transformative power is particularly interesting. Drawing upon Ruddick's idea of maternal thinking, he shows how recent debates in feminist theory do much to illuminate the complexities of power. In particular, power-over can involve a form of empowerment (e.g., the care and nurturance of mothering) even while it may be situated in a context of domination (e.g., patriarchy). Wartenberg is quick to underscore that mothering need not be limited to the family, not all nurturing is reducible to mothering, and that nurturance need not lead to empowerment, especially given that it may end up reproducing the dominant culture (and therefore reflecting the problem Wartenberg calls *superposition*). Rejecting Nietzsche and I. F. Stone's view of Socrates as an abuser of power, Wartenberg suggests that Socrates' elenchic interrogation provides an even better model of transformative power. Wartenberg notes that this model better highlights the importance of trust in a truly empowering relationship. Yet the empowerment may still remain elusive for a variety of reasons, including, most especially, the problem of superposition. We may trust nurturers to empower us, only to find they have enslaved us to the dominant order that constrains them.

Wartenberg has sought to fashion a specific theory of power while still insisting on the need to appreciate power in its plurality. Such an account of power, Wartenberg tells us, can inform criticism of specific social practices by highlighting the power relationships implicit in them. This is all done in a persuasive way, using important philosophical arguments to make insightful theoretical points. Yet Wartenberg's approach risks essentializing and reifying power at the expense of articulating the specific practices that confront actors in their lived experience in particularized settings. While Wartenberg stresses the variety of power relationships, forms, and uses, he still strives for a level of analytical abstraction that the diversity of power practices may not be able to sustain. His thoughtful reworking of the categories of power analysis, however, may very well provide new ammunition for the more localized battles.

Macalester College SANFORD F. SCHRAM

Freedom within Reason. By Susan Wolf. New York: Oxford University Press, 1990. 162p. $24.95.

Susan Wolf's concern is to offer a philosophical understanding of individual responsibility. She does so by examining two prevailing views of the character of individual freedom and agency and then developing one of her own. In doing so, she sheds considerable light on how and when we can attribute freedom and responsibility to individuals in a world in which their actions may be fully determined by psychological and physical causes. Those engaged in the study of politics can profit from her interpretation of these three views of human agency, particularly her argument regarding the conditions that must hold for the attribution of freedom and responsibility. Moreover, she manages to present them in a manner that is comprehensible to those uninitiated in the esoteric language of contemporary analytical philosophy.

Wolf's argument addresses perhaps the most critical and difficult philosophical problem faced by social and political scientists, that of reconciling individuals' freedom to choose with the deep ways in which social and psychological forces shape human behavior. As increasingly recognized by liberal political theorists, the presumption of substantial autonomy in moral and political decisions is brought into serious question by the "situatedness" of choice. Similarly, the presumption that individuals are able to make their own free and independent choices (at least some of the time and with respect to at least some important decisions) is not easily reconciled with models of explanation that view human behavior as fully determined by external causes. There is an obvious paradox in holding citizens and leaders responsible when external, "independent" variables are the basis for explaining what they do.

In Wolf's analysis, there are two prevailing views of the self: the *autonomy view* and the *real self view*. Those who argue from the autonomy view hold that to be free and responsible, individuals must be able to choose and act completely in the absence of external forces. By this view, social and physical influences must be ignored or overcome through an act of the will if choices are to be freely made. Proponents of the real self view, on the other hand, are willing to allow that individuals' beliefs and identity are fully shaped by social influences; yet, since these have been internalized in unique ways, the pattern of values and preferences that any individual has come to hold is properly seen as constitutive of her or his self. This is particularly true if the self is identified with a person's most deeply held values, rather than with the more superficial, transitory preferences or desires that he or she may hold.

Wolf identifies strengths and limitations with each of these views and, in a thoughtful and persuasive analysis, develops a *reason view* of the self that, she believes, is required for the attribution of freedom and responsibility. The critical element missing from both the autonomy and real self interpretations is the centrality of deliberation and reason in choices for which we hold

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individuals responsible. Such reasoning does not require autonomous separation from the world, nor is it excluded from a world that is fully determined by physical and psychological causes. Thus, liberal philosophers' search for independence of thought is unnecessary; and empiricists' explanation of political decisions and actions in terms of external variables does not, for that reason, deny freedom and responsibility to citizens or policy makers. Yet Wolf convincingly argues that individuals are able to reason only if they have been provided with the abilities, understanding, and resources for doing so. In particular, "an agent cannot have the kind of freedom and control necessary for responsibility unless, when making choices about values and actions, she can understand the significant features of her situation and of the alternatives among which her choice is to be made" (p. 117). In elaborating this requirement of responsibility, Wolf emphasizes the importance of normative competence—a competence that involves (among other things) reasoning from right values, rather than wrong ones, and true beliefs, rather than false ones.

To attach freedom and responsibility to one's ability to reason with reference to the true and the good, as Wolf does, is obviously a difficult notion for empirical political scientists and even political philosophers to entertain. Value subjectivism, in particular, is an assumption that is accepted almost without question among political scientists. Wolf understands the appeal of subjectivism, and she does a masterful job of challenging it. She suggests that there are no more compelling reasons for disallowing objectivity in values than there are for doing so with respect to facts. Conversely, reasoning is as central for determining good values as it is for forming what we accept as true facts and explanations.

These are not particularly novel ideas in contemporary philosophy; indeed, a valuable dimension of Wolf's book is her clear summary of arguments that in their original form, are virtually impenetrable to those who are not trained in academic philosophy. But they are ideas that empirical political scientists and political theorists need to encounter. More than this, there are compelling reasons for believing that something like Wolf's reason view of the self is essential for attributing freedom and responsibility. Even if this is not the case, she demonstrates that the autonomy and real self views that underlie most political inquiry offer insufficient bases for attributing responsibility to individuals for their actions. This calls into question normative and explanatory arguments to the effect that citizens and political leaders can be held responsible because their decisions have been made within the context of procedural rights and competitive elections or because they result from a process of agenda-setting and bargaining. If Wolf is right (and her argument is persuasive) our political principles and our explanations must demonstrate that to be held responsible for their decisions, citizens and leaders are provided with the normative competence they need to reason about the political problems and choices they confront even as they live in a fully determined world. Present political inquiry has a long way to go to make this claim.

Northern Arizona University LARRY M. PRESTON

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AMERICAN POLITICS

Judicial Compulsions: How Public Law Distorts Public Policy. By Jeremy Rabkin. New York: Basic Books, 1989. 322p. $22.95.

The Legacy of Judicial Policy-Making—*Gautreaux* v. *Chicago Housing Authority*: The Decision and Its Impact. By Elizabeth Warren. Lanham: University Press of America, 1988. 97p. $14.75.

Judging the Constitution: Critical Essays on Judicial Lawmaking. Edited by Michael W. McCann and Gerald L. Houseman. Boston: Scott, Foresman, 1989. 453p. $12.76 paper.

Though using varied approaches and contexts, each of these volumes focuses on recurring questions concerning the nature, role, and limitations of judicial policy making.

Jeremy Rabkin's *Judicial Compulsions* provides an interesting, though ideologically toned, argument against what he views as the consequences and implications of improper judicial intervention into administrative lawmaking. Emanating from the "legal legacy of the fevered 1960s" and "spurred" by the civil rights, antiwar, environmental, and consumer movements, Rabkin states that this development "encouraged a new approach to judicial controls on regulation, allowing beneficiaries of regulation ("so-called public interest groups") to sue agencies to demand more extensive or more rigorous controls (p. 4).

Despite elections that have placed in office more moderate and conservative presidents and members of Congress, Rabkin contends that "judicial controls developed in the heady days of the sixties" remain, penetrating judicial thinking and stirring judicial compulsions to intervene in administrative policy making. This is what encourages contending interests "to view even the details of policy as legally entailed and precommitted" (p. xiii).

Rabkin's major concern is that "such legalism is a distraction from the genuine policy issues that ought to dominate public debate about regulatory performance" (p. xi). Accordingly, the author sees the dire need to limit judicial power of courts and return policy making to officials who are politically accountable rather than rely on judges being used by private advocacy groups to achieve their objectives" (p. xii).

To support his argument, Rabkin develops rather detailed case studies of major policy concerns in three regulatory agencies: OCR, OSHA, and the FDA (chaps. 5–7). In the OCR chapter, for example, Rabkin details the protracted *Adams* litigation commenced by the NAACP Legal Defense Fund and its effects on the functioning and operations of the OCR. The suit was designed to compel enforcement of civil rights laws so as to overcome continued racial segregation in public schools. Rabkin argues that such litigation and "misguided judicial interventions" (p. 260) oftentimes frustrate and distort policy implementation, agency functioning, and our constitutional governing system.

This volume is certain to engender acclaim and criticism. It is likely to win praise from those who think the court's "misguided judicial interventions" do indeed distort and impair the policy process and proper institutional role and functioning. Likewise, Rabkin is likely to find strong support for the view that "traditional constitutional doctrines" overcome by the Warren Court in such decisions as *Brown* v. *Board of Education* (1954) and *Baker* v. *Carr* (1962), have led to "long term federal regulatory venture[s]" and "similar activist ventures in administrative law" (p. 129–30).

Such activist ventures impair "responsible government" and give public interest groups more policy leverage, leading to the "current system," which "has evolved from . . . a disoriented legal culture" reflecting "not simply the obfuscations of visionary law professors or the confusions of impulsive judges, but the underlying ambivalence in modern American liberalism." "To put it most simply," says Rabkin, "administrative law has sought to accommodate the desire for increasingly large government programs capable of rearranging society to serve consciously formulated plans, while at the same time seeking to accommodate deep-rooted suspicions of 'big government' or of centralized administrative power" (p. 243).

On the other hand, Rabkin's volume is also likely to engender some concerns. One limitation of the study, for example, as Rabkin himself seems to recognize (though with qualification) is that "the critical difficulty in all these cases would be to demonstrate that better policies would indeed have occurred *but for the intervention of the courts*" (p. 311, n. 20; emphasis original).

It is, of course, true, as Rabkin suggests, that "courts simply do not have enough power or enough expertise to supply the ultimate policy benefits that interest advocates seek" (p. 259). But neither do the president, Congress, or administrative agencies have enough power or enough expertise *on their own* to supply claimants such ultimate policy benefits.

A more overarching concern, however, is that Rabkin's study portrays a concept of constitutional governance that would somehow permit the Court to return to "traditional constitutional perspectives" (p. 33) and "the grounding assumptions of the traditional constitutional scheme" (p. 6). It would allow the Court to avoid "legalistic compulsions" (p. xiii) and limit judicial interventions to "more serious complaints about the substance of policy" (p. 268). Clearly, such normative criteria (about which reasonable persons might and do differ) offer minimal guidance or standards by which to determine, for example, when it is or is not appropriate for the Court to intervene or otherwise defer to politically accountable officials (see chap. 8).

The difficulty of reaching agreement on such matters (which are themselves the stuff of politics) would seem to suggest a continuing alertness to one of the most *fundamental* yet sometimes overlooked (or ignored) features of U.S. politics. This feature, so artfully developed by Jack Peltason in his pathbreaking *Federal Courts in the Political Process* (1955), suggests that it is not a matter of *their choice* but of *their function* (i.e., judicial review and statutory interpretation) that places courts and judges in the policy-making process.

Thus, whether viewed as liberal or conservative, interpretivist or noninterpretivist, or activist or self-restraining, the actions (or *inactions*) of courts and judges

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hold policy consequences that advantage certain interests and disadvantage others. This is the central factor that under particular circumstances leads interests to resort to litigation as political strategy.

Elizabeth Warren's *Legacy of Judicial Policy Making* also looks at the role and impact of the judiciary in the making of public policy. Warren's is essentially a lively case study of the development and impact of Chicago housing litigation that eventuated in the Supreme Court decision of *Hills* v. *Gautreaux* (1976).

In describing the context out of which the case developed, Warren captures well the situation in the mid-1960s that led to the "inevitable" collision between two "inexorable" forces: the determination of the growing Civil Rights movement to overcome racial injustice versus the "well-settled practices of the City government and the Chicago Housing Authority (CHA), backed by the hands-off policy of the Federal housing agency" (p. 11).

Particularly intriguing is the author's description of the four leading characters in this litigation drama. We start with Mayor Richard Daley, who wanted to—and in the main did—keep control and order in his city, which in effect meant maintaining racially segregated housing. Then there was federal judge, Richard Austin, who "privately concluded that he had been 'set up' by Mayor Daley and the Cook county machine as a 'last-minute gubernatorial candidate'" in a Republican year (1956) "and (as a result) his opposition to the machine and the Mayor was undiminished for the remainder of his life" (p. 5). More directly, as Warren put it, "Judge Austin and the Mayor became mortal enemies over the Gautreaux case" (p. 5).

Another leading figure in the litigation was Charles Swibel, chairman of the CHA for 19 years, who ran it "by fiat." Swibel, of course, "was loyal to Mayor Daley and saw eye to eye with the Mayor regarding public housing's place in City policy." Finally, there was Alexander Polikoff, an attorney who, with other attorneys from the American Civil Liberties Union, prepared and filed a class action suit in Judge Austin's federal district court on behalf of Dorothy Gautreaux. These were the lead players from which "the litigation received its stamp" (p. 4).

In chapters 3–5 Warren offers a vivid portrayal of the interaction of individuals, interests, and institutions (including the federal district court and court of appeals) in the drawn-out litigation that eventually led to the Supreme Court decision *Hills* v. *Gautreaux*, originally denoted as *Gautreaux* v. *Chicago Housing Authority*. Here the Court held that it was constitutionally permissible to fashion a metropolis-wide remedy, in that both Housing and Urban Development and the CHA had perpetuated illegal racial discrimination in the Chicago housing market and had necessary authority to make decisions on a metropolis-wide basis (which was the "relevant housing market").

Overall, Warren finds *Gautreaux* the "most successful of the various efforts to deconcentrate subsidized housing" (p. 91). In certain respects, however, Warren indicates some concern about the role, limitation, and impact of courts in policy making. That concern is both reflected and tempered by Warren's observation that today "there is reason to believe that judges are turning to experts for needed background information when they confront cases which deal with broad social issues. . . . A recognition of their own limitations and a desire to be fair makes such expert consultation an appropriate part of the judicial process" (p. 94).

In sum, Warren's is an informative, readable, and lively account of a continuing and important political–social problem and should certainly prove useful to the "students of public policy" (p. ix) for whom the book was principally written.

While Rabkin and Warren use relatively discrete contexts in which to consider fundamental questions concerning judicial policy making, McCann and Houseman's *Judging the Constitution* considers these and related questions from a rather broad range of theoretical and empirical perspectives. Here is a well-planned collection of excellent "original essays offer[ing] a variety of critical views concerning the character and implications of constitutional lawmaking by the Supreme Court in American public life" (p. 1). In a tightly written introductory chapter, the editors skillfully outline an overall context and organization that forges a unity and coherence seldom found in an edited collection of essays.

The essays assume, like the realists and the followers of the Critical Legal Studies movement, or "crits," "that rules and principles—whether rooted in the written text, the framers' intent, standing precedents, or ethical arguments—neither determine constitutional lawmaking in any mechanical sense nor impart to it any universal claims of ethical objectivity" (p. 6). But the essays are by no means doctrinaire. Indeed, unlike realists and some "crits," the authors, in the main, assume that "normal constitutional lawmaking practices remain substantively 'bounded' or constrained in important ways" (p. 6).

Additionally, while most authors view courts and law as potentially important to affect social change, they nonetheless recognize "the important limits to law as a force within society." This is illuminated brilliantly by Deborah Rhode's chapter, "Equal Protection: Gender and Justice." Rhode states that "despite changes in gender roles, the legacy of gender discrimination remains pronounced [indicating that] all too often equality in formal mandates has not translated into equality in actual status" (p. 281). And to Rhode, the persistence of such inequalities "suggests a general point about legal doctrine," namely, that "[while] law has been of critical importance in eliminating formal barriers to equal opportunity, it has been far less successful in challenging the informal structures, implicit assumptions, and social priorities that make such opportunities impossible to realize" (p. 281).

The volume is divided into five major parts, each part beginning with a brief overview of the respective contributions. Part 1, for example, "Contemporary Constitutional Theory," offers contributions on problems of legitimacy by Lief Carter, John Gilliom, and Judith Baier and "The Constitution and Social Change" by Stuart Scheingold and Rogers M. Smith.

Parts 2–5 focus on major areas of modern constitutional discourse and debate. Here, William Haltom and David Gray Adler offer essays dealing with how judicial review of the constitutional "separation and sharing of powers" scheme has affected subsequent institutional development. Part 3, "Property Rights and Economic Regulation," includes contributions by John Brigham and Margaret Jane Radin.

The chapters by Michael McCann, Deborah Rhode, and Rhonda Copelon in Part 4 focus on how judicial

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construction of Fourteenth Amendment equal protection has served to advance and limit progress by long-marginalized groups such as blacks and women. The volume concludes with contributions by Donald Downs, Roger Simpson, John Shockley, and Ronald Kahn offering critical assessments on important developments relating to the First Amendment (e.g., protections for speech, press autonomy, and religious practice and personal belief).

What makes this volume exciting and altogether worthwhile, is that though the various authors clearly hold particular and similar concepts about courts and law, they nonetheless offer an uncommonly sophisticated level of *critical* analyses of important contemporary issues. These analyses allow the reader to get a real feel for the nature, role, and limits of courts and law in U.S. politics. As such, the volume should appeal to a range of audiences, including students in graduate and advanced undergraduate courses.

Stanford University LUCIUS J. BARKER

LBJ and The Polls. By Bruce E. Altschuler. Gainesville: University of Florida Press, 1991. 137p. $19.95.

The Joint Press Conference: The History, Impact, and Prospects of American Presidential Debates. By David J. Lanoue and Peter R. Schrott. Westport, CT: Greenwood, 1991. 173p. $39.95.

These two books have little in common except an interest in the U.S. presidency. Lyndon Johnson, the subject of Bruce E. Altschuler's case study, declined to debate Barry Goldwater in 1964 and so warrants no more than a sentence or two in Lanoue and Schrott's volume.

Altschuler's thesis is that the Johnson White House commissioned, interpreted, and exploited polls in increasingly dishonest ways that reinforced, rather than dispelled, presidential isolation. Chiefly concerned with confirmation of their boss's popularity and political instincts, LBJ's minions are said to have engaged in shallow analysis of poll data while ratings were favorable and outright distortion afterward. A more objective reading of the polls, even if unwanted by LBJ himself, might have alerted the president to the limits of permissive consensus and tempered his policies accordingly.

Relying on primary materials obtained at the Johnson Library, Altschuler makes a strong case when recounting White House promotion of a preposterous poll taken in Vietnam, misrepresentation of results from a single-county survey in New Hampshire, and dubious definition of poll responses to exaggerate domestic support for the war in Vietnam. His indictment will ring true to many readers at a time when LBJ is held in especially low repute.

Some charges may not stick, however, since Altschuler contradicts several of his own arguments. A case in point is his criticism of Johnson's aides for not teasing more complex relationships out of the polling data. This complaint loses much of its force when Altschuler reveals that the data in question were often too limited for much exploration. Many, if not most, of the polls done for LBJ sampled opinion in one state, rather than nationwide; and few, if any, offered more than approval ratings, rough demographic groupings, and basic cross-tabulations. Much of this information, moreover, came to the White House free of charge as responses to items tacked onto polls otherwise unconcerned with presidential politics.

Other inconsistencies confound a rather simplistic account of how objective poll analysis could have restrained Johnson's Vietnam policy. Altschuler characterizes early public opinion on what to do about Vietnam as uninformed, ambivalent, but deferential to "any course of action that would work" (pp. 44–45). This was the permissive consensus Johnson enjoyed in 1965 when he embarked on a middle course between withdrawing from South Vietnam and all-out war against North Vietnam. (Altschuler faults White House analysts for not alerting Johnson to the limits of public support for such a policy but only two pages later recounts precisely such a warning by aide Hayes Redmon.) After escalation and subsequent loss of public support, Altschuler contends, LBJ should have heeded the polls and "reconsidered" his policy (p. 59). However, he later concedes that a sharply divided electorate allowed Johnson little latitude for radical change (p. 105).

On the one hand, Altschuler's account of Johnson's flawed strategy for contesting early primaries in 1968 is probably the most detailed available. His account of LBJ's relationships with national pollsters is also informative. On the other hand, several sketchy discussions of polling results cry out for supplemental tables or figures, especially since most of these data are not readily available to other scholars. More tables (or, better yet, a detailed appendix of questions and frequencies) would have enhanced the book's value as a source for others interested in the same issues.

Readers misled by Lanoue and Schrott's title into expecting attacks on the standard format for presidential debates will find, instead, a comprehensive inventory of theoretical frameworks, research methodologies, and empirical findings arising out of studies of most joint press conferences since 1960. David Lanoue and Peter Schrott skillfully weave most of these strands together into a schema theory and media interpretation framework for measuring the content and effects of future presidential debates. Chapter 2 sets the stage for all this empiricism by reviewing the highlights of every presidential and vice-presidential debate since 1960, and by recalling the conventional wisdom dominating journalistic interpretations of each event.

Widely believed since the first Kennedy–Nixon meeting in 1960, the most fundamental bit of conventional wisdom is that debates are important to election outcomes. Clearly, the acceptance of this notion can be discerned in the seriousness with which each team negotiates the technical details and then prepares its champion for the biggest television draw of the fall campaign. Another conventional wisdom of considerable vintage is that media scorekeepers wield enormous power over likely voters when they judge who won and lost the debate. The generally predictable pitches of campaign "spin doctors" immediately following each debate attests to the credibility of this thesis. A third piece of conventional wisdom dating back to 1960 is the conviction that debate watchers are more influenced by a candidate's personality, manner, and physical appearance than by the logic or substance of his arguments.

Lanoue and Schrott address these and similar notions in chapters 3–4. Chapter 3, a valuable discussion of the

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uses of content analysis in measuring type and substance of debate messages, establishes that candidate pronouncements are generally more substantive in debate presentations than in set speeches or media spots. (This may not be true of prenomination debates where more candidates have less response time.) After discussing consistency theory, functional theory, and other theories of attitude change, chapter 4 thoroughly reviews the literature of debate effects. Once they have apprised readers of differing operational definitions, Lanoue and Schrott conclude that presidential debates do importantly (if indirectly) affect perceptions of candidates, that this influence is filtered through media verdicts and peer discussions, and that people who did not watch a debate may alter their opinions of candidates in response to general reports of strong or weak candidate performance, but that the voting decisions of even favorably impressed viewers are not so easily changed.

In short, Lanoue and Schrott have written an essential reference work for any political scientist seriously interested in the subject. My only criticism is that their necessarily brief discussion of prenomination debates omits any consideration of how such multi-candidate and often acrimonious events might extend the negative effects of divisive primaries well beyond the states where debates are staged. This seems especially likely when long shots gang up on front-runners in hopes of winning more media coverage or when survivors of the winnowing process rip and tear at one another's accomplishments, judgment, and integrity. As Lanoue and Schrott point out, the format, numbers, strategies, messages, and audiences of prenomination debates are different from general election forums; effects may differ, as well.

Denison University EMMETT H. BUELL, JR.

The Cautious Welcome: The Legalization Programs of the Immigration Reform and Control Act. By Susan Gonzalez Baker. Lanham, MD: RAND Corporation and Urban Institute Press, 1990. 196p. $36.75 cloth, $19.00 paper.

Reflecting an ongoing tension between inclusionary and exclusionary approaches in U.S. immigration policy, the Immigration Reform and Control Act (IRCA), enacted in 1986, had two major dimensions: (1) benefit, or legalization and (2) enforcement. The key legalization provision, concerning general legalization, created a temporary program permitting status adjustment for undocumented immigrants to become citizens if they could demonstrate continuous residence in the United States since 1 January 1982. A second major provision permitted status adjustment of certain undocumented farm workers through (1) Special and (2) Replenishment Agricultural Worker Legalization programs.

The present study is part of a broader one of the implementation of IRCA, the Program for Research on Immigration Policy, undertaken by the RAND Corporation and the Urban Institute. It describes the design, implementation, and outcomes of these several legalization provisions of IRCA. It does not address the enforcement dimension, i.e., employer sanctions and increased border patrol. Drawing on interviews with those involved with program implementation and on firsthand observation, the report's overall assessment is that IRCA's legalization programs worked reasonably well (although the program has not been completed in that a large number of cases remain to be adjudicated).

The book's discussion of IRCA's legislative history underscores that granting citizenship to those who had entered the U.S. illegally was, and remains, controversial. That is why legalization was balanced by provisions for stronger enforcement. Also, the fact that there were several legalization programs, rather than just the general program, was part of a delicate balance to appease various interests, including agricultural interests concerned about continued labor supply.

Most of the book is given to a detailed recounting of the implementation challenges posed by the legislation and responses to those challenges. For instance, because of the need for effective outreach and publicity, the Immigration and Naturalization Service (INS) worked with "qualified designated entities" (essentially, local community organizations, church groups, and the like) to make undocumented immigrants aware of the legalization program. Yet these groups had traditionally been suspicious of the INS. There was also the irony of requiring documentation of continuous residence from undocumented workers. Baker's description of the approaches tried—and the levels of success achieved—in grappling with these and an array of other administrative, legal, and political difficulties makes for interesting reading.

While informative, because written as a report with an applied focus, political scientists are likely to find the work descriptive but not particularly analytical. For instance, the report indicates that in some locales and regions there were more or fewer applicants than had been estimated. The explanations offered for these, and other patterns and outcomes, however, may appear impressionistic or somewhat speculative. Similarly, those interested in the politics of policy formation will have to look elsewhere for a careful and extended discussion of IRCA's legislative history. Nor does the book seek to place its findings within any broad, or even middle-range, theoretical context; indeed, very few scholarly sources are cited, and there is no index. In short, the book's strength is in the *what*, not the *why*.

Baker's conclusion—that "change in the U.S. immigration system can be affected as much by reforming the benefits side as by modifying immigration law enforcement"—is presented in an all-too-brief closing section. It may well be that this conclusion is justified—that IRCA's legalization programs were somewhat successful; but the evidence marshaled in behalf of that argument may not convince everyone, and the standards for judgment that underlie it are not clear.

Thus, this report provides important evidence to assist in evaluating a major dimension of IRCA, and observers can learn much from it. However, the interpretation offered is neither conceptually well developed enough nor empirically well supported enough to be definitive. But this is due as much to the tremendous difficulty in evaluating a program as administratively complex and politically controversial as IRCA as to the assessment of IRCA presented in this volume.

University of Colorado, Boulder RODNEY E. HERO

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Deeds Done in Words: Presidential Rhetoric and the Genres of Governance. By Karlyn Kohrs Campbell and Kathleen Hall Jamieson. Chicago: University of Chicago Press, 1990. 275p. $27.50.

Playing the Game: The Presidential Rhetoric of Ronald Reagan. By Mary E. Stuckey. New York: Praeger, 1990. 127p. $37.95.

Politically Speaking: The Pragmatic Analysis of Political Language. By John Wilson. Cambridge: Basil Blackwell, 1990. 203p. $45.95 cloth, $16.95 paper.

All three books are about rhetorical strategies, persuasion tactics, and communication skills of public figures.

Deeds Done in Words and *Playing the Game* focus on the U.S. presidency and *Politically Speaking* draws some material from the U.S. presidency but more from the British parliamentary system.

All three works claim that rhetoric and communication have a vital role to play in understanding the "going public" part of political science.

Stuckey's book is the most narrowly focused of the three. It is a straight rhetorical analysis of Ronald Reagan's presidency. Stuckey's thesis is "[that] Reagan's use of language in his public speech was instrumental to the creation and maintenance of the "Teflon presidency," and that this use of language created a situation whereby the Teflon was bound to crack, as it did in 1986" (p. 1). Stuckey contends that "Reagan's rhetorical success was built around foreign policy events" and that this is why the Iran–Contra affair "provided the most conspicuous failure" of his administration (ibid).

Wilson's makes the most conservative claims of the three works. He subjects samples of political language to an analysis of linguistic pragmatics, a discipline "concerned with the way in which meanings are constructed" within a particular context (p. 3). One of the ultimate goals of pragmatic exercises in general is to gain "insight into communicative intentions" (ibid). In other words, pragmatists want to be able to say what the real motive and meaning of speech is, beyond what has been said. But their proof must follow established linguistic standards.

Wilson's goal, in contrast, is more modest. He wants to demonstrate that "pragmatic concepts are in operation in real talk, as opposed to contextually sterile examples frequently employed by linguists and philosophers" (p. 181). He argues that the analysis has implications, in turn, for the theoretical discussion of linguistic pragmatics. Since we learn from Wilson that linguistic pragmatics is in the throes of a controversy involved in defining the boundaries of the discipline, we should not be surprised that he devotes an entire book to one step in the defining process.

Campbell and Jamieson transcend the narrow rhetorical analysis of a single president's rhetoric and the conservative textual methodology of linguistics. Very simply, their goal is to demonstrate how speeches perform "vital functions for the preservation of the presidency as an institution" (p. 50).

Mary Stuckey presents good reasons for her contention that a scholar cannot understand Ronald Reagan's Teflon coating apart from a careful study of his rhetoric. But she may have pushed her thesis too far. Despite Stuckey's content analysis of Reagan speeches showing that he spoke more about foreign policy than about domestic policy in each passing year of his presidency, I remain unconvinced that the Teflon presidency was primarily a product of Reagan's articulation of foreign policy per se. Stuckey claims that the period 1983–85 was the Teflon period. Reagan's concentration on foreign policy made his image seem "durable and unassailable" because he could do with foreign policy what he did best rhetorically, that is, "communicate broad ideological parameters of an event or issue with little regard for the details or substance" (p. 87). But James Reston began to outline the Teflon presidency as early as 1981, when he wrote: "For the present it is generally agreed here that Mr. Reagan is personally dominating the news and the politics of this nation. He gets credit for everything that goes right, and . . . Haig or . . . Stockman get the blame for everything that goes wrong" ("A Sweet Fellow," *New York Times*, 8 March 1981). The point is that Stuckey may not have needed to prove the link between Teflon and foreign policy in order to establish her most important point, namely, that rhetorical analysis is an integral part of any serious study of Ronald Reagan's presidency.

I confess I had to work to follow John Wilson's intricately woven arguments. But then, I am no expert on pragmatic linguistics, and Wilson clearly has aimed this book at colleagues in his area of expertise. Nevertheless, there are tidbits in Wilson's work that scholars outside linguistics ought to find tasty indeed. Wilson argues that how politicians manipulate the various forms of *I*-pronouns and *we*-pronouns to present a particular perspective may tell us something about how the politician views issues and other people in and out of government in relationship to himself. Richard Nixon, for example, used *I* and *we* in a one-to-one ratio when he responded to questions about Vietnam. But when he responded to Watergate questions, he used *I* and *we* in a five-to-one ratio (p. 55). Also, Wilson says the choice to use *it* instead of *he*, *she*, or *they* potentially loads the subject at hand ideologically. The use of *you* and *one* may be cleverly designed distancing strategies. Wilson also explores why Nixon referred to himself in the third person as "*the president*," instead of *I*. The bottom line is that politicians use particular pronouns on purpose and that we ought to explore pronouns when we analyse rhetoric.

As a practical matter, the next step for Wilson—at the risk of stirring up even more controversy in his field—is actually to attempt some rhetorical analysis, using what he has learned. In other words, does linguistic pragmatics, when actually applied to real rhetoric, give us some more insight into the "going public" part of political science?

Deeds Done in Words is a book that was crying to be written in the field of rhetorical criticism. Campbell and Jamieson place presidential speeches into categories, or genres, and then distill the essence of those speeches to come up with certain principles that can then be applied to criticism of other speeches in the same genre. Genre criticism has been around for a long time, but Campbell and Jamieson are masters of the discipline. Although the authors answer their critics up front, the best test of whether or not genre criticism can be applied to presidential rhetoric is, Does it work? Do these genres tell us something about presidential rhetoric we did not know before? In this case, the answer is *yes*.

The genres Campbell and Jamieson have developed should keep students busy for a long time just applying the principles outlined to specific examples of presidential rhetoric. But the authors have done more. They have

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taken another leap forward in establishing the essential relationship between rhetoric and the presidency itself. They argue successfully that the genres they have identified actually help define what the presidency is and how that office interacts with the other branches of government and the American people. The rhetoric described by Campbell and Jamieson helps sustain the presidency as an institution. The speeches are real acts, "deeds done in words."

Although Campbell and Jamieson are very careful to provide good reasons why they have limited their book to certain genres of presidential messages—inaugurals, state-of-the-union and veto messages, war, impeachment, pardons, and farewell addresses—and even though they invite us to "explore this link in other forms of presidential discourse" (p. 4), I at least, await their next book. Defining other genres will be more difficult but very rewarding.

As the authors readily admit, their analysis ignores some rhetorically impressive and some historically important presidential messages, like Lincoln's Emancipation Proclamation and Gettysburg Address, Kennedy's "Ich Bin Ein Berliner," and even Reagan's remembrance of Normandy. And as complicated as it might seem to put into genre framework, we somehow need to account for policy messages, speeches announcing fundamental change in direction, institutional addresses to the people, the elusive press conference, and perhaps even the president as campaigner.

ABC News CAROLYN SMITH

Structures in Multiorganizational Systems. By Donald Chisholm. Berkeley: University of California Press, 1989. 196p. $37.50.

Organization Theory: From Chester Barnard to the Present and Beyond. Edited by Oliver E. Williamson. New York: Oxford University Press, 1990. 214p. $29.95.

"Organization matters" has become a catch phrase throughout the social sciences. Does it signal the development of a science of organization? In a collection of essays edited by Oliver Williamson celebrating the contributions of Chester Barnard, Williamson is cautiously optimistic; and with Donald Chisholm's work on informal organizations, caution need no longer limit optimism. Both books provide exciting glimpses of the progress made thus far in developing a general understanding of organizations.

Many authors, including Williamson, have emphasized the importance of hierarchy for achieving coordination. Chisholm, on the other hand, asks whether coordination is possible without hierarchy. His answer is *yes*. Chisholm's affirmative answer emerges from an examination of the San Francisco Bay Area public transportation system. This system is often characterized as chaotic because it is comprised of six primary operators and two supporting organizations with no overarching, unified authority coordinating the various transit providers. Each organization is unavoidably incomplete; in organization theory terms, each is an open system. The actions taken by one transit operator affect at least one, if not all, other operators.

In such a situation coordination may be necessary. But the standard solution—an overarching hierarchy—need not be. It all depends upon the level of interdependency among organizations. Bringing together within one organization a number of once-independent organizations may force *too much* interdependence and tax the cognitive limits of boundedly rational officials. Also, it is incorrect to assume that the lack of a single all-encompassing organization means that no coordination among interdependent organizations takes place. Coordination often does occur but informally—it does not appear on any organizational chart. Officials within different public organizations often develop out-of-channels contacts and agreements with one another. These informal organizations are not used for happy hour socials. Rather, they provide the means by which individuals can achieve organizational goals. Chisholm provides a number of examples.

The advantages of informal organizations as tools of coordination are many, as Chisholm convincingly argues. They are flexible and adaptive; they arise in relation to emerging problems, not in relation to demands of organizational designers; and they may minimize the complexities of interdependencies among organizations. In other words, they are a means of matching structures with environments in a discriminating manner.

Discrimination in matching structures with environments is a major theme emerging from Williamson's edited work. Williamson's contribution "Chester Barnard and the Incipient Science of Organization" provides a concise introduction to his own work, as well as a means of tying his approach to such stalwarts of organization theory as Simon and Barnard. Williamson shares Barnard's conviction that the black box of the organization must be opened. Organizations are consciously created as a means of allowing individuals to coordinate their actions toward a common goal, but when should coordination be attempted through hierarchies? And why hierarchies, rather than other coordinative structures? Briefly, Williamson's answers rest upon two fundamental concepts. First, Williamson borrows Simon's concept *bounded rationality* (there are limits to the cognitive capacities of individuals) and to it adds *opportunism* (individuals attempt to gain advantages for themselves). Hierarchies are a means of economizing on bounded rationality while safeguarding transactions from opportunistic behavior. Whether hierarchies should be used and what form they should take depend upon the characteristics of the transaction. Organizations, or hierarchies, should be matched with transactions in a discriminating manner.

In "The Politics of Structural Choice" Terry Moe asks how Williamson's approach can be extended from private sector organizations to public bureaucracies. The key, according to Moe, is to adopt Williamson's concepts but not the *questions* for which they have been put to use (which have been primarily economic).

Instead, by recognizing political features of the decisionmaking environment, such as public authority, political uncertainty, and political compromise, political (not economic) explanations for the emergence, shape, and performance of political (not economic) structures can be developed. This would involve interest groups, legislators, executives, and bureaucrats bargaining and haggling over structural solutions to common problems (p. 129). Moe thinks that political scientists should no longer relegate themselves to studying the politics of bureaucracy, while leaving questions of organizational

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structure to other disciplines. A science of organization that would address interesting political questions requires that the political and the structural be joined.

The thought-provoking work of Chisholm, Williamson, and Moe warrants optimism concerning the development of a science of organization. However, a number of fundamental issues must be addressed before a full-blown science emerges. The schism between the *rational systems* and *natural systems* approaches to organizations must be addressed. Rational systems theorists, such as Williamson, view organizations as being purposefully designed to achieve particular goals. Natural systems theorists focus on the non-rational character of organizations and their creators (e.g., Levitt and March's "Chester I. Barnard and the Intelligence of Learning"). As W. Richard Scott ("Symbols and Organizations") points out, Barnard was both a natural systems and a rational systems theorist. How to bring both strands together in a single science of organization is not clear. Most often, scholars from each approach talk past each other (as demonstrated in these essays). In addition, as Mary Douglas argues ("Converging on Autonomy") culture should be incorporated into a science of organization in order to explain the origin and change of individual preferences instead of assuming them fixed.

While a full-blown science of organization has yet to emerge, both books are highly readable and thoughtful examples of exciting steps taken in that direction by social scientists struggling to understand why organizations matter.

University of Arizona EDELLA SCHLAGER

The Imperial Middle: Why Americans Can't Think Straight about Class. By Benjamin DeMott. New York: William Morrow, 1990. 260p. $18.95.

Making Sense of Politics. By Arthur Sanders. Ames: Iowa State University Press, 1990. 217p. $24.95.

It is commonly observed that U.S. citizens do not think much or deeply about politics. The cacophony of man-in-the-street interviews, radio "talkback" programs, and morselized "factoids" from public opinion polls do not generally provide a very hopeful view of most citizens' comprehension of the political world. Arthur Sanders's in-depth investigation of citizens' opinions about the political world, *Making Sense of Politics*, tends, on the whole, to confirm this assessment. Benjamin DeMott's book, *The Imperial Middle*, provides one explanation of why. Although DeMott's thesis explicitly concerns the erasure of class as a device to explain political outcomes, the mechanisms he describes (particularly the media) can be usefully extended to explain the trivialization of politics more generally.

Sanders's book continues the tradition of Smith, Bruner and White, Robert Lane, and more recent scholars like Jennifer Hochschild and Doris Graber by offering an analysis of citizens' thinking about politics based on intensive interviews with a small (n = 26) randomly selected cross-section of residents of Utica, New York. This method can yield useful insights about individuals' understanding of politics that might easily be overlooked in the conventional public opinion survey. The drawback (Sanders notes almost too frequently) is that the small sample size and variability across interviews makes it impossible to generalize with any confidence about the patterns of relationships uncovered among the subjects. Given these limitations, Sanders manages to do a creditable job of systematically describing the common themes that emerged from his interviews.

The major difficulty with Sanders's study is that while his intention is to probe beneath the surface responses obtained in the large-scale survey setting, his perspective is very much limited by the analytic concepts popularized by this tradition. Sanders's findings concerning the limited understanding of ideological terms and lack of perceptions of differences between the parties echo those from large-scale survey research. His focus on the differences between stylistic and policy-based understanding of the terms of political discourse, however, provides additional insight into the question of why such self-designations tend to predict some political attitudes and behavior even though common understanding of terms like *liberal* and *conservative* is so limited.

The material developed on the centrality of political trust in individuals' judgments about politics is substantially more valuable. Sanders develops a simple typology of patterns of trust based on beliefs about the political system and attitudes toward individuals. Within this typology the attitudes of the *skeptics* (who believe that the system is corrupt but that individuals are basically honest) are worth some attention. Three themes emerge from the investigation of these attitudes. The dominant one has to do with the pernicious influences of money in the campaign process. The second has to do with the perception of the increasing isolation of the politician from the experiences and needs of the contitutents at home, and the third with distaste for compromise and log-rolling. The intertwining of these themes, together with subsequent chapters on the role of personal experience and common sense in shaping citizens' views of politics, does much to explain the popularity of simplistic solutions like the current movement for term limitations.

Sanders's investigation of the themes of freedom, opportunity, and equality provides a window on the thought processes of the "average" citizen that tends to validate the concerns expressed in DeMott's book. However, it is surprising to find a chapter on the values of freedom and equality in politics that makes no reference to Milton Rokeach (*The Nature of Human Values* 1973). While a few of Sanders's subjects (4 of 26) tended to value equality of outcome, and a few *libertarians* (five) tended to value freedom, the dominant frame of reference expressed in the interviews concerned equality of opportunity. The dominance of this notion of a fair chance at the starting gate is the central concern of *The Imperial Middle*.

The citizens that Sanders interviewed are clearly unaware of the concept of class as an explanation for political outcomes. They have all accepted the notion that they are part of the "imperial middle"—not a class but a mass of strivers. If they succeed, they deserve to enjoy the fruits of their success; if they or others fail, it is most likely their own fault. Even those of Sanders's subjects most concerned with equality are disturbed by the unfairness of people being given things they did not work for. According to DeMott, the well-buttressed myth of classlessness immobilizes society and makes it

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impossible to deal with clearly evident crises in policy-making: "America's faith in classlessness simultaneously whitewashes unearned advantages" (p. 229). The myth is reinforced by the media (particularly popular entertainment) and by the schools.

DeMott observes that the term *class* has been dismissed from modern language to be replaced by the hegemony of "the imperial middle," to which rich and poor alike seek access. In modern culture the concept *class* has been redefined and trivialized as a rip-off run by prestige peddlers—an advertising gimmick—that wise citizens are capable of recognizing as a sham. Status is temporary and various—no longer fixed but defined by movement and flux. The masking language of class relies heavily on the euphemism *life-style*—giving the illusion of individual choice and reinforcing the ideal of mobility and individual responsibility.

The myth of classlessness is reinforced by the "omni syndrome"—which is reinforced in turn by a multitude of examples of "downward access." If the well-off and advantaged can enjoy the entertainments and life-styles of the "Joe Six-packs" of the world, then, obviously, Joe can step up (if he cares to). But in modern entertainment, Joe is too wise to want to. He can see through the illusions and glitter. It is more fun to watch "Lifestyles of the Rich and Famous" on television than to have to take care of all that fancy stuff.

DeMott's explanation of the role of the school in reinforcing this myth is particularly enlightening. In school everybody starts off with a fair chance. Failure in school is seen to be the result of a lack of intelligence, laziness, or poor judgment. As Sanders's subjects illustrate, the personal experience of the egalitarian school atmosphere reinforces the societal myth that personal error is responsible for lack of success. Both school and mass media work together to replace structural explanations for social and economic outcomes with psychogenic ones that degrade the individual product of the working-class environment by dismissing aspects of experience and culture that provide a different framework from which to understand moral choices in the context of limited opportunities. (DeMott's reanalysis of the motivations of Mary Beth Whitehead [Baby M's genetic mother] is particularly enlightening here.)

DeMott argues convincingly that media constraints reinforce the taste for simplistic thinking rather than educating the public about the complexities of policy decisions and social truth. He proposes the "double truth" of social responsibility coupled with individual responsibility as the necessary context in which to view policy solutions. It then becomes the intellectual responsibility of those who have a part in making policy decisions to reject both sentimentalism and judgmentalism in favor of the complexity that gives each factor its appropriate weight. Properly done, this might prevent the continual vacillations of policy committments driven by underlying assumptions that assign blame for failures wholly to society during some periods and wholly to the individual during other periods. DeMott argues for complexity and balance. It remains to be seen whether such can be achieved in a culture dominated by sound bites and quick fixes.

University of Houston KATHLEEN KNIGHT

The Descent of Icarus: Science and the Transformation of Contemporary Democracy. By Yaron Ezrahi. Cambridge: Harvard University Press, 1990. 354p. $35.00.

The Bittersweet Century: Speculations on Modern Science and American Democracy. By Paul N. Goldstene. Novato, CA: Chandler & Sharp, 1989. 220p. $22.95.

For all their pervasive impact on human life in the twentieth century, science and technology remain alien and forbidding territory to most political scientists excluded from the customary domains of scholarly investigation. It is refreshing, therefore, to see two new books that take on the challenge of integrating science into the analysis of U.S. democracy. Though markedly different in style and substance, these books together send a welcome message that science and technology carry too much explanatory force to be ignored by contemporary political theory.

Yaron Ezrahi's capacious study looks across some four centuries of cultural history to find connections between science and technology and the forms of liberal–democratic politics. The practice of submitting scientific claims to public witnessing during the Enlightenment and the corresponding decline in the authority of private and esoteric knowledge made it possible, in Ezrahi's view, to reconcile democratic participation with centralized political action. Modern science, with its emphasis on visual, experimental demonstrations of truth, transformed the relationship between the governors and the governed. A *celebratory* visual culture, in which the monarch was endowed with superior claims to knowledge—a status symbolically affirmed by the king's all-seeing position in the Stuart court theater—gave place to what Ezrahi terms an *attestive* visual culture based on a presumed equality of perception between the performing agent and the spectator public. Science and technology thus irrevocably altered the space for public action, requiring political actors to be held accountable against norms of objectivity, impersonality, and representativeness. In a scientifically conditioned culture that dissolved hierarchical differences and credited all viewers with equal capacity to discern reality, an instrumental paradigm for politics naturally flourished, for the success or failure of political actions could now be assessed with reference to common understandings of facts and the truth.

To demonstrate these parallels in the evolution of scientific and political culture, Ezrahi musters a kaleidoscopic array of evidence from the history and sociology of science, art and architectural history, sociology, and political theory. His intellectual range is extraordinary; and if the reader feels captive at times on a compulsory tour of the major monuments of Western social thought, the vista is seldom uninteresting, the guide is as literate as he is learned, and the argument is by turns compelling and provocative.

The author's talents are splendidly in evidence in a sequence of chapters that link the experimental strategies of scientists like Robert Boyle, Joseph Priestley, and Michael Faraday with the attestive orientations expressed in the works of contemporary painters, poets, and political theorists. Ezrahi shows how the culture of attestation won particularly influential adherents among U.S. political thinkers, from the federalists' espousal of a "science of politics" to the U.S. pragmatists' adoption of the "republic of science" as a model for democratic political behavior. The United States' love affair with

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action over thought and experience over inference was reflected, Ezrahi argues, in a social science tradition that rejected Durkheimian notions of the duality between individual and society in favor of a view of public order arising from the mutual discipline of multiple individual actions.

Coming to the end of the twentieth century, however, Ezrahi sees a disintegration of the United States' commitment to the attestive culture of Enlightenment science and its political implications. Developments within science are partly to blame. Thus, Ezrahi believes that the discovery of irreducible uncertainty in physics has fundamentally weakened the power of science to legitimate political action. If the attestive eye can no longer establish the reality of physical events, still less is it capable of verifying the consequences of social or political action. Science has become "privatized" in the sense of ceasing to be a potent resource for public political discourse. In place of the earlier attestive culture, there has arisen a new *reflexive* visual orientation, wherein the act of seeing (whether with the human eye or the mechanical eye of the camera) shapes that which is seen and the observer becomes a constitutive element of the observed "reality." Ezrahi attributes to the decline in the public authority of science a variety of shifts in late twentieth-century U.S. politics: a loss of faith in instrumental public action; a preoccupation with short-term, aesthetically gratifying political objectives; a rise in eclectic pluralism; and heightened criticism of expert authority.

A serious flaw in this otherwise admirable study is Ezrahi's tendency to treat science and technology as wholly independent variables, rather than as forces that both influence, and are influenced by, politics. The latter view is much more in keeping with current work in the history and sociology of science, and the failure to represent it is a curious blind spot in a book that generally makes such sophisticated use of these sources. The attempt to pull the analysis into the present is also fraught with problems; for here, as the author recognizes, "the absence of an historical perspective limits our ability to interpret recent experience and distinguish significant from marginal developments" (p. 239). Recent experience, in fact, leads one to question the more optimistic, as well as the more pessimistic, strains in Ezrahi's concluding chapter. In the aftermath of a "Nintendo war" in the Gulf, sustained by high levels of public enthusiasm, one wonders whether the attestive visual culture and the manipulative instrumentalism it supports are not alive and well, at least in the sphere of high-technology military action. Perhaps more significant is Ezrahi's omission of environmentalism as a possible counter to the privatization of science; this newly powerful political ideology is not only deeply (if ambivalently) rooted in science but also appears to be stretching out the timespan for legitimate instrumental action in Western democracies. One misses in this context any discussion of the possible globalization of the attestive culture through such transforming visual images as the fragile and lonely planet Earth suspended in space.

Loose ends are more conspicuously evident in Paul Goldstene's book, which starts with some of the same premises as Ezrahi's but comes to startlingly different conclusions about the impact of science and technology on politics. The "bittersweet century" of Goldstene's title serves as the leitmotif for a series of short, reflective essays decrying the United States' deviation from a truly egalitarian democratic ethos. Liberalism, Goldstene contends, has foisted on the United States a system of elite rule founded on the superior claims to knowledge of the few and supported by the corporate organization of technology. In positing an alliance between liberalism and technology, Goldstene acknowledges that the applications of science can be appropriated by politics but evades the larger implications of this argument by drawing an artificial boundary between science and technology. Like Ezrahi, Goldstene associates science with the core assumption that all individuals are equally situated in their capacity to know. Unlike Ezrahi, however, he believes that science is on the point of exerting a redemptive force on U.S. politics by liberating it from the tyranny of elite rule. Goldstene draws messages of optimism from modern biology (which has undermined the basis for racist and hereditarian thought) and from relativity and quantum physics (which have subverted positivist and privileged claims to knowledge). "The logic of science," he concludes, "results in democracy" (p. 153)—a sweet promise that offsets the for him bitterly elitist impulses of the twentieth-century United States.

A lack of specificity and a partial reading of the record of science make these arguments highly problematic. Advances in biology, for example, may indeed have eroded the basis for nineteenth-century racist views and the naive eugenics of the early twentieth century. But genetics, molecular biology, and sociobiology are reinjecting a note of determinism into explanations of human behavior that is all the more persuasive because its scientific foundations are regarded by many as unimpeachable. In ascribing a democratizing effect to modern physics, Goldstene overlooks a point of which Ezrahi is acutely conscious—that theoretical progress in this field has assailed the value of science itself as a way of knowing reality. Nor is there much support in current historical or sociological studies for the boundary Goldstene seeks to construct between science (which he idealizes as a creative striving for knowledge) and technology (its bureaucratic and corporately structured counterpart). Ezrahi comes closer to prevailing opinion when he treats science and technology as equally important resources in supporting the instrumental paradigm of liberal–democratic politics. There is, finally, little in Goldstene's account to explain how the democratically invigorating force of science will win out over the existing "technobureaucratic" organization of U.S. politics. Without such explanation, and with only weak connections to recent theoretical studies of science and technology, the book remains little more than a wistful, fin-de-siècle footnote beside Ezrahi's more enduring work of scholarship.

Cornell University SHEILA JASANOFF

Laws, Men, and Machines: Modern American Government and the Appeal of Newtonian Mechanics. By Michael Foley. London: Routledge, 1990. 284p. $47.50.

Michael Foley has thoroughly demonstrated the extent to which U.S. scholars, journalists, and political leaders, as well as the public, utilize the language of Newtonian mechanics to analyze and evaluate political institutions. Specifically, Foley points to the centrality of

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the concept of *balance* in discussions of U.S. political institutions, particularly the separation of powers. Foley demonstrates that the manner in which the separation of powers is routinely treated "has given the separation of powers every appearance of being a real physical system composed of discrete units interconnected with one another to produce an enclosed pattern of direct physical contact, in which the constituent parts change in response to the forces exerted upon them by the others" (pp. 72–73). In short, inherent in the discourse citizens use to discuss the separation of powers is the paradigm of Newtonian mechanics.

Interestingly, in making the case for the influence of the Newtonian paradigm on U.S. political discourse, Foley is not arguing the influence of Newtonian ideas on the political thought of the framers of the Constitution. Indeed, he suggests that debates regarding the relative impact of Newtonian ideas on the framers' political thought are "not only inconclusive, but largely unproductive" (p. 50). Foley analyzes contemporary treatments of U.S. political institutions and reaches the conclusion that the idea of balance, understood in Newtonian terms, is inherent in the U.S. political culture. Indeed, he suggests that the "cult of balance" is "the predominant and guiding conceptualization of American government" (pp. 225, 233).

The significance of the Newtonian paradigm was made most apparent, Foley illustrates, during the "imperial presidency" crisis of the 1970s. After decades during which the growth of the presidency was described, explained, and justified in biological or organic terms (i.e., as a vital institution adapting to changing conditions), the imperial presidency crisis brought back to the fore the language of mechanical balance; that is, the crisis was widely interpreted as an imbalance of power, a deviation from some ideal balance of power between Congress and the presidency that would almost automatically (i.e., mechanically) be recaptured through the resurgence of congressional power. In a final chapter, Foley suggests that the idea of a mechanical balance also pervades U.S. discussions of the judiciary, federalism, the federal bureaucracy, the mass media, and other institutions.

Foley is not interested in evaluating the utility of the Newtonian paradigm as an analytical tool or as a standard of political evaluation but merely demonstrating, through remarkably extensive citation of academic and popular sources, the extent to which the paradigm dominates U.S. discourse concerning national political institutions. Herein lies both the strength and the weakness of *Laws, Men, and Machines*. Foley's thesis is unchallengeable, but its significance demands exploration.

It is beyond question that the separation of powers is nearly universally talked about as a kind of mechanical balance that historically tilts to one side or the other before some equilibrium is achieved. But what are the consequences that flow from conceptualizing the separation of powers (and, e.g., federalism) in these terms? What are the implications of utilizing the idea of balance as a standard of political evaluation? Foley does not address these questions.

What are the normative commitments that underlie the mechanical conceptualization of U.S. political institutions? With what view of democracy, for example, is it most consistent? What sorts of political reforms benefit from the mechanistic paradigm? To what extent are the sorts of political reforms suggested by Lloyd Cutler and the Committee on the Constitutional System, who propose to create a more parliamentary style of government hindered by the "cult of balance"? In short, what is the political significance of the cultural attachment to Newtonian mechanics?

It may be unfair to criticize an author for what he has neglected to write. However, it seems clear that the flaw of *Laws, Men and Machines* is not that Michael Foley is wrong (for certainly, he is not) but, rather, that he fails to demonstrate the significance of what he has found.

University of Texas, San Antonio JOHN R. BAUER

Boston against Busing: Race, Class, and Ethnicity in the 1960s and 1970s. By Ronald P. Formisano. Chapel Hill: University of North Carolina Press, 1991. 323p. $34.95 cloth, $12.95 paper.

When violence erupted in Boston in the early 1970s as school buses transported students pursuant to a federal court order to desegregate the public schools, the nation was shocked. How could this citadel of culture, home to over a hundred colleges and universities, the launching point of the American Revolution, and a hot bed of antebellum abolitionist thought, become a bastion of resistance to African–American claims of civil rights and equality under law? The initial understanding, which has taken on the aura of conventional wisdom, is that resistance came from racist Irish–Americans in South Boston and Italian–Americans in East Boston. In *Boston against Busing*, Ronald Formisano offers a richly textured explanation that accents racism, of course, but also class and ethnicity as key variables in understanding what happened in Boston. His account is a substantial contribution to the literature not just on Boston but on school desegregation generally.

While earlier commentators focused on extremist whites, Formisano seeks to understand the attitudes, beliefs, and strategies of the "silent majority"—whites who opposed racial busing but did not resort to tactics of violence and civil disobedience. Formisano sympathetically portrays white middle- and working-class parents who believed in the rule of law and were prepared to send their children to desegregated schools but who feared—reasonably at times—that violent clashes would occur and resented the political and economic powerlessness that left them unable to change the law or send their children to private schools.

Formisano's analysis of the tactics and strategies of the antibusers suggests that they ironically built their campaigns on lessons learned in the African–American protests of the 1950s and 1960s and in Vietnam War protests of the 1960s. In this connection, he highlights the important role of women in the antibusing movement. Although these working-class women shared few values with their more affluent sisters in the Women's movement, the antibusing movement launched a number of women on career paths that took them away from their previous role as homemakers.

Formisano joins a chorus of voices that looks rather bleakly at the overall experience with school desegregation. In Boston between 1973 and 1987, the reality of "white flight"—parents either moving to suburbs or placing their children in private schools—resulted in a decline in the percentage of white students in the Boston public schools from 60% to 26%. Not surprisingly, this

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process left behind lower-class white students in school with lower-class black students. When economic conditions permitted, black parents also chose to remove their children from the public schools.

By recognizing the complexity of antibusing sentiment, Formisano sheds light on the role of economic class in school desegregation. In Boston, affluent suburban whites supported the policy objectives of desegregation, knowing full well that their communities would remain rigidly segregated: "The lower classes did the desegregating, the middle classes did the fleeing, . . . while the affluent were exempt from the start" (pp. 232–33).

He is less persuasive in demonstrating the role of ethnicity in the school desegregation crisis. He broadly characterizes certain traits as "Irish," "Italian," and so on. However, just as antibusing sentiment was not homogenous, neither was Boston Irish society. Ardent Irish supporters of desegregation included not only U.S. District Judge W. Arthur Garrity, Jr. but also Senator Edward Kennedy and many civil rights attorneys. In West Roxbury and Hyde Park, the antibusing response was quite different from that of predominantly Irish South Boston, even though Irish–Americans constituted a substantial segment of each community.

An intriguing element that Formisano does not elaborate is the role played by law and the legal system—particularly federal law and federal judges—in the Civil Rights movement. He notes that elected state officials typically refused to take any responsibility for the process. Looking at the larger canvas of the national Civil Rights movement, federal judges who are appointed for life played an enormously instrumental role in desegregating various public facilities. Their independence (by virtue of their lifetime positions) allowed them to act in a way that was not beholden to petty politics or local concerns.

University of Arizona College of Law ROBERT JEROME GLENNON

Extraordinary Measures: The Exercise of Prerogative Powers in the United States. By Daniel P. Franklin. Pittsburgh: University of Pittsburgh Press. 172p. $24.95.

U.S. citizens pride themselves on possessing a government "of laws, not men." Yet our national history is crowded with examples of extraordinary actions taken by the officers of the republic in response to challenging circumstances from Lincoln's conduct of the early Civil War to the internment of Japanese–Americans during World War II to the aggressive exercise of judicial review by federal courts in recent decades. These actions and a host of others are based on the prerogatives of the state and its officials, rather than explicit constitutional grants of power. As such, they pose a problem for the U.S. political system, says Daniel P. Franklin in this brief volume. "How," he asks, "can plenary power be reconciled with democratic principles, and how can citizens be protected against governmental abuse of authority in times of emergency?" (p. 3). Franklin does not provide a complete answer to his own question but, instead, offers a primer on the issue of prerogative powers in the U.S. constitutional system. His book is a valuable, well-written introduction to a subject that has received insufficient attention in the literature on U.S. politics.

Franklin begins by noting that there are essentially two ways of dealing with the issue of prerogative powers in the United States. The first is to see them as extraconstitutional. This position maintains the image of pristine constitutional government ("of laws") while accomodating the need for "extraordinary measures" in times of emergency. The other view is that the Constitution is big enough and flexible enough to encompass unwritten prerogative power, and that, as a nation, we are safer with such authority inside rather than outside the constitutional domain. Franklin clearly advocates the latter position, makes a case for it, and examines what it means for government.

His argument stands on two legs. First, the founders of the U.S. republic saw the traditional royal prerogative recognized in medieval political theory and European practice as invested in the new U.S. regime (moving, as he puts it, "from king to country"). Second, the alternative, extraconstitutional view of prerogative is dangerous because it makes such powers illimitable. Therefore, if we are to understand our system, he believes it incumbent on us to examine the nature and limits of prerogative powers held by the different branches of government under the Constitution. He not only reviews the well-trodden ground of presidential prerogative but explores the comparative wilderness of congressional and judicial prerogatives.

Franklin's discussion of presidential prerogative is valuable in two respects. First, while it only scratches the surface of this subject (which has received extensive attention from political scientists, legal scholars, and historians) it does highlight most of the major issues involved. But, more importantly, it also shows how foreign policy prerogatives, administrative prerogatives, and even rhetorical prerogatives fit together in a larger picture of presidential power.

The book opens new areas for most readers in its discussion of congressional and judicial prerogatives. Franklin shows how a number of congressional powers and practices really stem from prerogatives and must be understood as such—namely, the investigatory power of the legislature, the use of legislative vetoes (continued even in the face of contrary court decisions), and even control over certain internal procedures. For the courts, the major prerogative of interest to Franklin is judicial review. His discussion of it, within the larger context he is considering, offers a useful perspective on an important topic.

The book's brief survey of these issues leaves the reader wanting more. Perhaps the chief flaw of the book is that a number of subjects are either insufficiently covered or dismissed in a perfunctory way. For example, Franklin never really examines the alternative (extraconstitutional) view of prerogative. Nor does he examine congressional control of internal procedures in sufficient detail. Moreover, there is not as much structure in the internal organization of his chapters as I, at least, would have liked.

At the same time, however, the reverse of these flaws constitutes the book's chief virtue. It provides a brief, readable introduction to an important topic; a global perspective on a subject that is usually treated in a much more fragmented fashion; and an extensive (albeit in-

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complete) set of citations for further reading and research. It is a book that anyone seriously interested in U.S. politics ought to read.

Miami University (Ohio) RYAN J. BARILLEAUX

The National Party Chairmen and Committees: Factionalism at the Top. By Ralph M. Goldman. Armonk, NY: M. E. Sharpe, 1990. 649p. $49.95.

This is the only book I know of that so thoroughly details the history of U.S. party leadership from the beginning of the republic to 1960. In Goldman's view, this history teaches at least three general lessons: (1) that politics and political partisanship are basically about the management and socialization of conflict; (2) that political factionalism at the top generally has roots in the electoral foundations of the party system; and (3) that federalism is often a solvent of party, making party nationalization a slow process that is accomplished only tenuously in the late twentieth century.

Adopting E. E. Schattschneider's view that politics is the management and socialization of conflict, Goldman focuses virtually all of his attention on the history of intraparty conflict that has plagued the life of the national committees. In this connection, the author points out that the roots of party faction existed from the beginning of the republic in spite of a homogeneous electorate consisting mainly of property-owning white males. Given an electoral foundation for consensus, one would think there would be little room for a fiercely partisan politics at the top. However, even though the Federalists were conceptually antiparty and refused to take grass-roots organization seriously, that did not prevent the emergence of two rival and cohesive leadership cadres centered around Hamilton and Jefferson. True, these early "parties" amounted to little more than personal networks. But the absence of fierce partisanship in the electorate did little to generate consensus among elites. Hence, while Goldman tends to suggest that the grounds for factional division at the top are generally traceable to the characteristics of the party electorate, this is not universally true. The early party coalitions remained fluid and personality-based at least until the Civil War when sectionalism became the basis of organized factions.

But sectionalism was also responsible for breaking apart organized factions, suggesting that party is not always the solvent of federalism that V.O. Key suggested it was 30 years ago. During realigning periods, groups at the top splinter and resettle with the formation of new issue cleavages in the electorate. Drawing our attention to the decentralization of the U.S. party system, the book contains many stories about how party factions in state organizations eventually translated into factions at the national level.

In addition, we learn that the national chairmen of each party were always the creatures of a state party system (usually New York, Pennsylvania, or some other politically dominant state). As such, "the state origin of chairmen was undoubtedly related to the facts that these states were also major sources of presidential candidacies, state party influence at the national conventions, and, in victory, major recipients in the distribution of federal patronage" (p. 554). That the northern industrial states controlled the party leadership for much of history explains why the Republican party was never supplanted by the Progressive insurgency of the early twentieth century, and why Democrats weathered the Populist movement: "The degree of national committee contact with state organizations often depended upon which was the dominant state faction" (p. 567).

Not until the 1920s did the parties begin to consolidate power at the top. Factionalism did not cease, but it did seem to lose some of its sectional character. Simultaneously, at the electoral level, the 1920s marked a temporary reconsolidation of the party system. But for the first time, the national party organizations seemed to have acquired an independent existence, apart from state-level activity. Issues had become national in scope and the universal impact of the Great Depression would only reinforce this nationalization of party politics. Faction within the parties now had a different foundation. Rather than sectional splits, intraparty conflict was now based upon divisions between the executive and legislative branches (chap. 19).

Again and again, a familiar pattern of party organizational surge and decline appears in Goldman's history. Democrats, as the out-party, unify to elect a president. Then, once the goal of winning is no longer there to unify, intraparty strife sets in over issues and spoils. Republicans, now the out-party, struggle to reunify, recapture the presidency, then dissolve in strife and turmoil.

This regular series of peaks and troughs corresponds closely to electoral trends in party support. Whether the causal direction runs from electoral behavior to elite behavior or the other way around is not always clear from Goldman's chronological treatment. Except for the early period of party formation, it appears that elites act in response to local and sectional electoral pressures. The initial impetus for the formation of a Democratic National Committee in 1848 was the need for party leaders representing different regions to consult and contest presidential issues and candidacies (p. 556). But it is still true that factions that stubbornly obstructed party cohesion have had regional electoral roots at least up until the 1920s and 1930s. It is just not clear that the national committees succeeded in containing faction on their own, as exogenous actors in the party system. The rare periods of intraparty cohesion appear to be coincidental to periods of electoral stability, rather than the accidents of inspired leadership or the result of institutional developments in the national committee bureaucracy.

With these kinds of questions in mind about the relationship between electors and elites, I believe that this book is a natural companion volume to histories of U.S. electoral behavior, such as James Sundquist's *Dynamics of the Party System* (1983). What is missing is a treatment of national party developments since the 1960s.

Goldman concludes on a controversial note, arguing that the national committees have grown in size, permanence, and political importance (p. 576). But while the committees may be better staffed than ever, that does not mean they are politically more important. All trends indicate that in many respects they are less important, even as they have become more bureaucratized. While the national committees are more independent of state and local organizations, at the same time, they have lost influence with the emergence of independent party organizations that have developed within the House

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and Senate and with increasingly independent candidates that have turned to their own organizations for fund-raising and support.

Finally, the Goldman account does not address the issue of national committee response to divided government, a post-1960 phenomenon. It is just not as simple as in-party versus out-party any longer. Is the Republican National Committee currently an in-party or an out-party? Winning the presidency without winning at least a closely competitive margin in the Congress does not make an in-party. Today, with comfortable margins in the House and Senate, Democratic chairman Ron Brown is able to conduct himself very much as if he is running the in-party. Hence, these recent developments complicate the discussion, making it difficult to pronounce such confident judgments about the relevance of an in-party/out-party understanding of national committee power.

Senate Labor Committee JAMES GIMPEL

Culture and African American Politics. By Charles P. Henry. Bloomington: Indiana University Press, 1990. 140p. $22.50.

Black Americans' Views of Racial Inequality: The Dream Deferred. By Lee Sigelman and Susan Welch. New York: Cambridge University Press, 1991. 214p. $22.95.

These two very different books on black politics have a common theme, namely, the diversity and complexity of black political attitudes. The controversy over the nomination of Clarence Thomas for the Supreme Court is just one demonstration of the importance of this theme.

The Sigelman and Welch book, a secondary analysis of national survey data on black political attitudes, examines a nontraditional topic using traditional methodology. In contrast, the Henry book uses a very nontraditional approach, analyzing folklore, blues, and black churches to identify the cultural roots of black politics.

Sigelman and Welch make a major contribution to our understanding of racial attitudes. Relying heavily on the 1981 and 1986 ABC News–*Washington Post* telephone polls of blacks, the authors find a continuing massive perceptual gulf between blacks' and whites' attitudes toward racial inequality. Blacks and whites differ dramatically in the amount of prejudice and discrimination they see and in their explanations of this racial inequality. Blacks do not believe the playing field has been leveled, while many whites insist that it has been or has even been tilted toward blacks, who are "taking over."

This study of the racial gap, which receives less media attention than the gender gap, fills the scholarly void with a well-written, clear, concise examination of perceptions of the amount of prejudice and discrimination against blacks, explanations of why blacks are worse-off than whites, and preferences for policies to end racial inequality. Synthesizing previous work while both confirming and challenging expectations, it should be widely used by those who want to probe more deeply into these topics.

Elaborating on the two separate but overlapping worldviews, Sigelman and Welch argue that blacks see themselves largely as victims of white-dominated society. Seeing white hostility toward blacks as widespread, 60% of the black respondents say they have been affected by at least one type of discrimination in education, housing, jobs, or wages. Two, three, four or even more times as many blacks as whites perceive white discrimination. Similarly, blacks typically point to structural sources of discrimination, while whites downplay their own responsibility for black problems.

Sigelman and Welch also argue that the attitudinal differences among blacks are as important as the differences between blacks and whites. Most blacks do not forswear all black responsibility for inequality, nor do most whites forswear all white responsibility (p. 107). But blacks and whites differ among themselves in the weight they give to structural and attributional–dispositional factors. The primary variable associated with differences in explanations of racial inequality is the extent of perceived discrimination against blacks. There are also differences in black attitudes toward busing, affirmative action, and aid to blacks and the poor; but blacks are generally more activist than whites. Both blacks and whites, are less enthusiastic about government programs than they were during the 1960s.

The authors are commendably explicit about the limitations of the available data and the measures and methods of analysis, acknowledging the coarse measures of discrimination and their inability to make precise comparisons over time. We need to know much more about variations over time and those associated with regions, local contexts, racial consciousness, party identification, and level of political activity. Sigelman and Welch's book cries out for replication and elaboration.

Charles Henry's book is also an ambitious work, distinguished by the crossing of disciplinary lines. His search for the cultural roots of contemporary black political behavior, actually concentrates, after a chapter on African American folklore and the blues, on the way black churches and black leaders have incorporated and modified the cultural roots.

His main argument is that African American politics has a distinctive style and combination of worldviews. The black style, in contrast to mainstream U.S. politics according to Henry, includes a wider variety of political expression and demand, emphasizes identity and self-respect that cross class lines, depends on rhetoric and charisma, and is rooted in a black church tradition that blends sacred and secular vision.

The argument is disjointed, and the use of material from other disciplines does not do justice to earlier scholarship on African American culture. Nevertheless political science will benefit from this broader exposure. Critiquing earlier studies of black leaders for neglecting the participatory component in their rhetoric, Henry says the church-based militant tradition was the catalyst for the Civil Rights movement that transformed black politics. He argues that black cultural styles mean that important political action occurs outside the voting booth in revolts, protest, and direct confrontations in the tradition of "black bad men" (p. 48).

The root of both Martin Luther King's and Jesse Jackson's success, according to Henry, is the black church tradition's emphasis on faith, collectivism, suffering, and authoritarianism and linking of the Bible's moral lessons to current political events. Drawing parallels between Marcus Garvey's and King's efforts to construct a civic religion, Henry argues that Jackson's

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campaigns were a reassertion of Martin Luther King's protest style of leadership stressing self-respect, self-help, and faith in the future. Henry challenges Adolph Reed's criticisms of Jackson's antidemocratic and organic style, saying that Jackson represents "a logical extension of black cultural politics with system transforming potential" (p. 86).

Henry concludes by affirming that black culture has empowered blacks to survive oppression and promote a community of interests and criticizing black neoconservatives for obscuring this vision. According to Henry, black politics is linked both to U.S. politics and to the struggles of oppressed groups worldwide. Questions can be raised about his interpretations of both black political culture and mainstream U.S. political culture but not about the importance of the questions he is addressing.

Both these books contribute to correcting some of the limitations of traditional political science by changing the nature of the discourse. In stimulating more complex and comprehensive theories and models, they demonstrate again the way the issue of race is transforming not only U.S. politics but also the discipline of political science.

Wellesley College DALE ROGERS MARSHALL

The Changing World of the U.S. Senate. Edited by John Hibbing. Berkeley: Institute of Governmental Studies Press, 1990. 403p. $24.95 paper.

Elections, Representation, and Congressional Voting Behavior. By Robert A. Bernstein. Englewood Cliffs: Prentice–Hall, 1989. 117p. $18.60.

Research on the functioning of political institutions has been one of the most important topics of study in U.S. politics. Do institutions represent the views of those governed? How have the internal operations and level of representation changed over the course of the nation's history?

Two recent books provide very different perspectives on this subject. An edited volume on the U.S. Senate compiled by John Hibbing presents a series of high-quality articles by many of the leading scholars in the field (based on an October 1988 conference at the University of Nebraska, Lincoln). These articles deal with several general subjects: the historical development of the Senate, internal operations, and elections and representation.

Part 1 is must reading for all those interested in the historical evolution of the upper chamber. It has chapters dealing with reconstitutive change in the Senate from 1789 to 1841 (Swift); party unity from 1880 to 1920 (Brady, Brody, and Epstein); and party voting from 1877 to 1986 (Hurley and Wilson). Because these three pieces are so well written and clearly conceptualized, they perform a valuable service in the field of congressional studies. The history of Congress in general has been neglected by political scientists, and these chapters go a long way toward filling that gap.

Part 2 emphasizes the internal operation of the Senate. Among its articles are materials on party leadership (Patterson), leadership institutionalization (Canon), multiple referral (Davidson), unanimous consent agreements (Smith and Flathman), and televising of Senate proceedings (Fenno). The Fenno piece is particularly interesting because of its attention to a part of the congressional process (the internal media) that has clearly grown in importance in recent years. Fenno uses the Senate debate over television to examine institutional change and members' arguments about the type of institution that is desired.

Part 3 has a number of very important discussions about elections and representation. It includes articles on policy voting and representation (Wright), California Senate races (Jacobson and Wolfinger), the role of challengers (Squire), House members running for the Senate (Copeland), the dynamics of Senate races (Stewart), split party control (Oppenheimer), and campaign spending (Abramowitz). Wright's article presents a novel use of state-level data to evaluate senatorial responsiveness, while Jacobson and Wolfinger use the 1980 and 1986 California Senate races to demonstrate the importance to incumbent success of strategic framing in the campaign.

The final group includes a series of roundtable essays by Polsby, Ripley, Fiorina, and Matthews. These articles integrate the research findings of earlier chapters and point the direction for future work. The essay by Polsby presents an informative reflection on the changing role of the Senate. Fiorina calls for more comparative research and the need to be more explicit about the theoretical underpinnings of House–Senate differences.

Bernstein's is a very different type of book. It is what I would call a "message" book because of its clear enunciation of a simple theme, which he calls the "myth of constituency control." In this logically argued and concise book, he argues that citizens rarely know or respond to policies supported by their representative, that constituencies rarely reward or punish incumbents on the basis of policy preferences, and that members therefore are not controlled by constituencies.

One strong point of this book is its review of a considerable amount of the research that has been done on the subject of constituency control. Bernstein has done an admirable job of synthesizing a diverse range of material on public opinion and legislative voting decisions. At times, though, the author equates constituency control with issue control. Almost all the evidence gathered relates to constituency control through the communication of policy views.

This ignores recent arguments (e.g., the economic voting thesis) that constituents can engage in democratic control through more broad-based types of evaluations. If voters can exercise significant control using retrospective judgments, citizens may not be as poorly armed as Bernstein suggests. In addition, new models suggest that members can be controlled through the anticipation of citizen preferences. Arnold, for example, demonstrates that anticipated reactions can be as important to members of Congress as more commonly studied forms of after-the-fact control.

But for scholars interested in a concise and readable review of past research on representation, this book is very useful. It is well written and clearly argued. The author deserves credit for his careful attention devoted to testing key hypotheses in the representation area. The volume can be assigned in introductory courses on elections and Congress.

Brown University DARRELL M. WEST

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The Reach and Grasp of Policy Analysis: Comparative Views of the Craft. By Richard I. Hofferbert. Tuscaloosa: University of Alabama Press, 1990. 192p. $19.95.

Defining policy evaluation as the use of social science research to measure under what conditions public programs achieve their objectives (p. 4), Hofferbert draws upon his experience as an academic policy researcher and student of policy analysis in developing countries to examine the opportunities and problems confronting this process.

Part 1 reviews the contemporary history of U.S. policy analysis since the halcyon "social engineering" years of Lyndon Johnson's Great Society and examines the activities of university-based policy research using cases with which Hofferbert is directly familiar. Chapters 2 and 3 provide useful examinations of extension research in land grant universities and practical challenges facing university researchers engaged in public-funded program evaluation research. It is possible to derive from these chapters hypotheses that could be tested through surveys of university and other research centers. Part 2 examines the obstacles and opportunities confronting the export of U.S. policy analysis methods and epistemological assumptions to the analysis of developing nations (particularly Turkey and Nepal), using previously published research.

Hofferbert argues that political scientists engaged in policy research engage in either *product* or *process* research. While the former focuses on policy performance and is useful to legislators who desire independent means of evaluating agencies, the latter has greater long-term utility to program administrators. Moreover, process research also stands the best chance of endearing the discipline to policy analysis consumers, since it examines the relationship between the organization of authority as an independent variable and the accomplishment of policy objectives.

Hofferbert asserts that the "green years" of Great Society optimism minimized the long-term relevance of the work of policy analysts cloistered in universities and maximized their political vulnerability when social policies did not meet political expectations quickly or decisively enough. This has resulted in the institutionalization of policy analysis in places out of sight of many political scientists. Within state and local government institutes, agricultural extension centers, and similar places, however, policy research has flourished.

While there is much to warrant such an interpretation, rising overhead to sustain policy research in traditional academic departments, the colocating of some policy analysis fields (e.g., environment and energy) into multidisciplinary research centers, and the fact that the social problems being analyzed were larger than initially envisioned, as noted by observers such as John Schwarz in *America's Hidden Success*, (1988) may also have contributed to the apparent decline in support for social-science-based policy research.

Whether using firsthand experience in the evaluation of elderly services in New York state or applying contemporary policy analysis to evaluating Turkish economic development, the book is at its best when it sticks to its pragmatic theme—that effective process analysis requires a basis of trust between researcher and policy practitioner, as well as frequent consultation (p. 19), adaptation of research methods to field conditions, and resolution of tension between a disciplinary reward structure that often appears to penalize "practical" research.

Unfortunately, Hofferbert does not explore the phenomenology of effective political science participation in policy research. As Hofferbert's cases illustrate, even university-based policy analysis is often conducted in interdisciplinary environments where project research agendas are established by several disciplines and methodologies and approaches are defended before committees unfamiliar with the idiosyncrasies of the researcher's discipline. Yet there is broad appreciation for the political variables responsible for policy success or failure. How are problems defined in such a context? How are research strategies developed to target these political variables in these environments effectively? This omission is ironic, since Hofferbert does explore the independence of political variables in successful policy implementation in developing countries with pluralistic political participation.

Other topics salient to the subject are also omitted, such as the rise of policy think tanks, both liberal and conservative, giving rise to rapid evaluations of existing public programs. The rise of these institutions may, in part, explain why, despite the decline in pure social science research dollars for studying social programs, the amount of policy analysis output (sometimes masquerading as objective scholarship) still arguably increases. One wishes that Hofferbert had generalized his analysis to permit a weighing of factors intrinsic to policy evaluation success, particularly in his discussion of extension research at land grant colleges where problem-oriented research has been conducted by social scientists in natural resource, economic development, and technology transfer policies. Taken as far as his cases permit, Hofferbert provides a good starting point for exploring these issues.

Oak Ridge National Laboratory DAVID LEWIS FELDMAN
University of Tennessee, Knoxville

Race and the Decline of Class in American Politics. By Robert Huckfeldt and Carol Weitzel Kohfeld. Champagne–Urbana: University of Illinois Press, 1989. 205p. $27.50.

The central arguments of this book deserve close study by both political scientists and political strategists. In addition to an account of the rise of race and the decline of class and income distribution as fundamental organizing themes of U.S. electoral politics, the authors develop original approaches to these problems: how to secure the representation and fundamental rights of blacks and other minorities, blunt and alleviate racial polarization in the party system, secure more effective representation of class, and prevent the withering away of the Democratic party.

An unusual book, replete with mathematical models and computer simulations (many readers will have tough going there), it also offers many pragmatic suggestions for restructuring the system. The analysis incorporates historical accounts and well-displayed quantitative data and is driven by a profound concern for the quality of democracy in the United States. It has much to offer on all these fronts. Another strength is the bringing together of political vision, quantitative theory, and

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data; flaws of style in an often-well-written book are a result of the ambitiousness and unwieldiness of that effort. In addition, it will anger many people because it cuts into the premises on which their political experiences, positions, identities, and loyalties are based.

The empirical foundation of the book is the thesis that "race has persistently obstructed the development of class politics across more than one hundred years of American political history" (p. 188) and is often "a wedge that disrupts lower-class coalitions, thereby driving out class in our political arrangements" (p. ix), so that "the decline of class as an organizing principle in contemporary American electoral politics is directly related to the concurrent ascent of race" (p. 2)—with the result that race is now the most important line of conflict in U.S. politics, while the politics of class has been virtually eclipsed.

The political eclipse of class is dangerous: "So long as a society is class-structured, democratic politics must also be structured by class if it is to have legitimate meaning and content" (p. 191). The increasing significance of race over class politics was not the substitution of one legitimate dimension of conflict for another: "Unlike class, race is not an appropriate basis for the organization of democratic politics because, in a democracy, both parties must be concerned with the egalitarian treatment of blacks and white. . . . Both the Democratic and Republican parties must be parties of civil rights" (p. 191).

The fact that 9 out of 10 blacks support the Democratic party has led many whites whose racial attitudes make them uncomfortable in a party that gets a significant part of its support from blacks or that emphasizes racial issues to leave the party. Racial polarization of the parties provides such people a reason for leaving the Democratic party on racial grounds. Because defections by whites in many settings are greater in number than conversions to it by blacks, the Democratic party has been severely disadvantaged.

Thus, the racial polarization of the parties leads to a system in which it continues to be rational for the Republican party to make negative racial appeals. If blacks were somewhat more evenly distributed between the parties, following their now-more-diverse class interests, then the parties would be more competitive, both would have less incentive to arouse racial fears and animosities, and both would have to guarantee civil rights. And blacks *would* distribute themselves more evenly between the parties (in the authors' scenario) if party appeals were oriented more toward class, and less toward racial, issues.

The authors argue that the racial polarization of the parties was not, and is not, inevitable. The racial structure of the contemporary party system is a result of a racial strategy that had surfaced earlier but was given enormous impetus by Lyndon Johnson in the 1960s. A class-based Democratic party strategy could in time reverse the structuring forces set in motion by racial issues. Some strategies and structures of party competition give ready expression to racism; others do not. Even in a society in which racism is common, a more strongly class-based politics could appreciably reduce the political role of racism.

The authors' model of party competition is likely to prove very useful. Because the eclipse of class by race varies greatly, depending on party strategies and on the particular mix of blacks and whites in the parties in a particular location, the model is rooted in the characteristics of locations (e.g., precincts, states, the nation). Some combinations of strategies and groups, if allowed to run on without intervention, lead in the model to the virtual demise or enduring minority status of one of the parties; other combinations lead to more or less stable and vigorous party competition. By constructing a locational model, the authors have opened the way to viable, low-cost but theoretically fruitful empirical research on the politics of race and class in many settings.

Many blacks—and whites—will find it very difficult even to consider seriously the suggestion that affirmative appeals to race, as well as negative ones, should be moderated or that more blacks should support the Republican party—even less the notion that the Democratic party and the party system can be rescued by such steps. No doubt, these are hypotheses that can be tested finally only in the doing. In the meantime, the authors of this book have provided us with theory and data that change our understanding of the possibilities and their consequences. Their analysis of race and party system justifies, from an important perspective, the shift in party strategies predicted by Kevin Phillips in *The Politics of Rich and Poor* (1990). While their book can not settle the questions it raises, it raises them in an especially fruitful way that should guide future research and perhaps (as its argument becomes more widely known and understood) political action, as well.

San Francisco State University RUFUS P. BROWNING

Television and the Crisis of Democracy. By Douglas Kellner. Boulder: Westview, 1990. 287p. $48.50 cloth, $14.95 paper.

After two decades of steady media criticism from the Right, *Television and the Crisis of Democracy* is the latest in a series of books from the Left attacking network television in general, and news coverage during the Reagan administration in particular. The author's thesis is that "network television is the shame of the nation today—a complacent, compliant servant of power. Rather than promoting democracy, robust public debate, and an informed public opinion, the networks diminish and weaken U.S. democracy. They also promote the conservative interests that own and control them, thus precipitating a massive crisis of democracy and journalism in the United States today" (p. 173).

While remaining true to its leftist roots, this book offers a comprehensive literature review that dutifully includes mainstream research. An extensive bibliography covers the spectrum of media sociology and political communication. The author offers a hegemony model more comprehensive and multidimensional than standard Marxist and neo-Marxist theoretical analysis and less rigid than instrumentalist approaches. Within this theoretical framework, the author posits that three powerful corporations attempt to control leisure, entertainment, information, and mass communication in the interests of corporate profit and social control.

In a well-written historical section on the rise of network television, Kellner convincingly describes the forces that made U.S. television the most commercialized in the world. While most countries in Europe held steadfastly to a noncommercial model, U.S. citizens

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were racing toward an unbridled commercial system controlled largely by monopoly interests. Kellner contends that the overcommercialization of radio helped promote a homogeneous consumer culture and established the hegemony of national networks over local affiliates.

Much of the criticism of the news coverage of the Reagan administration is similar to that found in Mark Hertsgaard's *On Bended Knee* (1988) and Martin Lee and Norman Solomon's *Unreliable Sources* (1990). Kellner argues that television and the media were key factors in protecting the Reagan administration from "complete collapse" after the Iran–Contra affair and in the election of George Bush. The author's criticism of the media's lack of coverage of alleged behind-the-scenes negotiations between Reagan campaign operatives and the Iranian government in 1980 over the timing of the hostages' release seemed to be highly prophetic. In an appendix, Kellner fully details the media's shortcomings in covering the October Surprise, offering details on which the media failed to focus until mid-1991.

In a chapter on alternatives, the author argues for a more pluralistic system of broadcasting. He proposes a substantial strengthening of the public broadcasting system, the resurrection of the National News Council, free airtime for political candidates, and a congressionally mandated study of media concentration. The author uses his own experience producing programming for a public access channel in Austin, Texas as a primer for more effective citizen participation in the media system.

This book goes to great lengths to build up the networks to mythic proportions at precisely the time they are in steep decline. Descriptions of network affiliates as "vassals" under the control of "television overlords" are already dated as today's affiliates wage open rebellion against their weakened masters (p. 73). A publication date of 1980 rather than 1990 would have given this book more credibility. Even a decade earlier, however, the notion of the all-powerful television networks was fraying around the edges.

By tenaciously sticking to his thesis of network hegemony, the author foreclosed a thoughtful examination of the political balance in the postnetwork era. There is no discussion of whether the decline of the networks and the ascendance of cable have benefited the Right or Left or how "narrowcasting," globalization, and the future convergence of media might affect the political balance of power. Other authors have already accounted for the changing electronic media marketplace, and their works are better for it.

Southern Illinois University, Carbondale JOE S. FOOTE

Crimes of Obedience: Toward a Social Psychology of Authority and Responsibility. By Herbert C. Kelman and V. Lee Hamilton. New Haven: Yale University Press, 1989. 382p. $35.00 cloth, $14.95 paper.

On 16 March 1968 Lieutenant William Calley led the second platoon of Charlie Company in an assault on My Lai 4. It was later revealed that as many as four hundred elderly Vietnamese civilians and children had been systematically slaughtered by Calley and his men. Social psychologists Herbert C. Kelman and V. Lee Hamilton view the My Lai massacre as a frightening example of the threat posed by lockstep compliance to authority and explore the social psychology of obedience in this new volume.

Their work is one of several recent books exploring various dimensions of obedience to authority and marks a much-needed shift in emphasis. For almost two decades social scientists have been infatuated with why people (collectively) rebel while ignoring the equally vital question of why people obey. Relatively little systematic work has been devoted to analyzing the well-documented human capacity for cooperation and acquiescence in venal and morally questionable governmental policies and programs. Even less effort has been devoted to understanding those solitary nonviolent resisters who, historically, have at great personal risk defied authority and forced us to admit that "the emperor has no clothes." The questions of obedience and resistance (the authors observe) have taken on even greater currency and import with the involvement of "the best and the brightest" in Watergate and Iran–Contra and with the rampant crime in the suites. It is increasingly apparent that state terror and the abuse of power are not the exclusive preserve of immoral individuals. Instead, such acts are typically bureaucratic crimes that result from the coordinated efforts of otherwise normal people who dutifully fulfill the expectations and dictates of their superiors. In short, to paraphrase Pogo, "We have met the enemy and it is us."

Those seeking emotional power and moral potency may encounter difficulty wading through Kelman and Hamilton's concern with theory building, scale construction and data analysis. At times, the authors' prose resonates with awkward and dense jargon. Their book also suffers from a glaring lack of coherent transitions and organization. They provide the pieces, but the reader must be sufficiently deft to put the puzzle together. Nevertheless, Kelman and Hamilton have made a major, albeit flawed, contribution to the psychological literature on governmental crime and immorality. (They focus on crimes of obedience that they consider to be illegal or immoral acts committed in response to the orders or directives of authority [p. 46].) They have provided intellectual perspectives and insights which promise to guide and inspire future sojurners in this relatively uncharted area.

The initial portion is a familiar rendition of various contemporary cases of governmental excess and crime. Chapter 3 provides an unexpected sketch of traditional and contemporary religious justifications for disobedience to the dictates of authority and the "superior orders" defense in the law of war. These chapters unfortunately remain like the dark side of the moon. They are of import; but the authors' fail to explore their practical significance and terrain fully. Once having disposed of these obligatory matters, Kelman and Hamilton turn to a theoretical discussion of regime legitimacy and draw heavily on Stanley Milgram in enumerating the psychological processes facilitating and inhibiting disobedience. These chapters provide familiar but valuable insights that unfortunately are not satisfactorily integrated into the volume.

The core of the book is the authors' analysis and discussion of a 1971 national survey and a refined version of the survey administered to a Boston sample in 1976. These surveys explored respondents' attitudes concerning whether they would have participated in the My Lai killings, their perception of how others would have behaved, and the justifiability of prosecuting and

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penalizing William Calley (who was ultimately paroled) and the others involved in My Lai. The authors distinguish respondents based upon their willingness to exonerate—versus assign responsibility to—Calley, his unit, and his superiors. Kelman and Hamiltion ultimately explain the respondents' differing attitudes based upon three divergent orientations toward national government: rule, role, and value. Those with a *rule* orientation believe they are obligated to follow the dictates of authority. Weak, vulnerable, and fearful, such individuals find security and safety in adherence to their leaders and superiors. *Role*-based citizens view themselves as good citizens who support the U.S. government. They are emboldened by the regime's strength and prestige, which provides them with a sense of pride and accomplishment. These two ideal types would probably overwhelmingly accept the familiar jingoistic credos My Country Right or Wrong and America, Love It or Leave It. Disobedience is centered in the rather small segment of the population characterized by a *value* orientation. The support of this highly educated group (which appears to range between roughly one-fifth and one-third of the population) depends upon the regime's conformity to the norms and values that these moralists believe undergird the political system. The authors, to measure these three orientations, develop scales that likely will be refined and employed in the future by researchers studying compliance and disobedience.

Kelman and Hamilton, of course, analyze attitudes rather than behavior. Nevertheless, their general conclusions mirror those of other researchers who have pointed to the United States' alarming tendency to elevate patriotism over principle. The inclination is to exculpate both those in the trenches (and, to a lesser extent, government leaders) of responsibility. Only the autonomously minded are able to withstand peer pressure, ridicule, and harassment and assert that right and might are not necessarily synonymous. The authors offer a laundry list of institutional reforms and innovations to cultivate democratic dissent. It is unlikely that such proposals will be seriously considered or discussed. We will probably persist in feting disobedients abroad while flagellating them at home. Dissenters will continue to be dismissed as kooks, crackpots, and criminals. The irony is that the passion of resisters historically has provided a vital fuel for powering the engine of our democracy. Meanwhile, educators might ponder the fact that 51% of the respondents in Kelman and Hamilton's 1971 sample replied they would obey orders to shoot all the Vietnamese inhabitants of a village suspected of aiding the enemy. Thirty-three replied that they would refuse to shoot (p. 173).

University of Illinois, Chicago MATTHEW LIPPMAN

Procurement and Public Management: The Fear of Discretion and the Quality of Government Performance. By Steven Kelman. Washington: American Enterprise Institute Press, 1990. 213p. $26.50.

Steven Kelman's new book has two topics. The narrower topic, the procurement of computers by the federal government, will strike most people as a bit soporific. But the broader topic, the role of rules versus discretion in the public sector, is a central issue to anyone interested in public management, public policy, or organization theory.

Kelman's theme is that government procurement is far more rule-bound—and leaves far less to discretion—than the same process in the private sector and that this is a terribly costly arrangement. He shows how common sense is constantly perverted by the requirements of the bidding process.

The problem is not, as one might think, that federal computer purchasers are required to offer contracts to the low bidder. Other factors may be taken into account and often are. The problem is that purchasers may *not* take into account anything that cannot be clearly specified in the bid documents—including vendors' reputation in the marketplace and even their past performance at the same agency. One of the most depressing findings in this book is that those involved in federal procurement actually take pride in their ability not to be "biased" by this vital information.

While the perception, says Kelman, is that government needs more competition in the bidding process, the reality is that private-sector procurements are in general far *less* competitive. Instead, private purchasers seek ongoing relationships, which not only yield valuable information about performance but, more importantly, motivate the vendor to do a good job.

By requiring competition on written criteria, the federal government eliminates any incentive for vendor performance above the contractual minimum, such as finding innovative solutions to problems. Kelman cites the example of a vendor who proposed a cheaper and more effective equipment configuration in a bid, only to be entangled in a scandal because the purchasing agency awarded it the contract instead of reopening the bidding using the new specifications.

All this boils down to a point that is now familiar to readers of the organizational economics literature: it is impossible to write complete contracts under conditions of uncertainty. This book illustrates the point very clearly and with a dismaying wealth of examples. Dedicated bureaucrats struggle to define in advance what will constitute good performance by vendors; but their best efforts are doomed to failure, since no one can anticipate all eventualities.

The alternative, Kelman sensibly suggests, is to give bureaucrats more discretion so that they can exercise judgment. He argues that the risk of corruption can be minimized by modern investigation technology (including, he cheerily notes, wiretaps) and by having a relatively open process requiring consensus among a number of individuals. A conservative reform would be to incorporate judgments of vendors' past performance as an explicit evaluation factor. A more radical reform would be to give multimember panels essentially complete discretion in awarding contracts, requiring only that they state their reasons in writing.

Kelman makes a good case for reform. No one reading this book could possibly think that things are just fine the way they are. As a piece of policy analysis, this is a strong effort. As political science, it is less satisfactory—and at times frustrating. Why, exactly, do we give so little discretion to public officials? Kelman answers this question only briefly, but his approach will be familiar to readers of his earlier work: U.S. citizens have a culture that simultaneously holds high expectations of public officials and views them with acute suspicion. As a result, we enmesh them in restrictions and are intolerant

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of the slightest deviation. The fault is not in our bureaucrats but in ourselves.

If this is the whole story, it is relatively easy to be optimistic about reform: we just have to stop being so silly. But is it the whole story? Consider that other great web of constraining rules, civil service, which many public managers would consider a far more important problem than procurement restrictions. A look at history shows that civil service was not born of a cultural whim but of a bitter and drawn-out battle. Most significantly, it was viewed as a way to control not bureaucrats but politicians. The same may be true for procurement: if restrictions are based on a well-founded fear of misbehavior (specifically political entrenchment) by politicians, then reform becomes much trickier.

Moreover, there are winners, as well as losers, under the current system. Kelman alludes briefly, for instance, to some people in Congress who are deeply suspicious of any more discretionary process that might benefit IBM. Who are these people, and what are their interests?

Kelman also tends to use *government* as a synonym for *federal government*. But there is considerable variation among local governments in their personnel systems and, presumably, of procurement also. How can this variation be explained? A better understanding of these sorts of questions would take us far toward understanding why bureaucracy looks the way it does.

University of Arizona HOWARD FRANT

The National Security Constitution: Sharing Power after the Iran–Contra Affair. By Harold Hongju Koh. New Haven: Yale University Press, 1990. 340p. $35.00 cloth, $14.95 paper.

Constitutionalism, Democracy, and Foreign Affairs. By Louis Henkin. New York: Columbia University Press, 1990. 125p. $25.00.

Is our eighteenth-century Constitution fit to govern a modern democracy? More specifically, in foreign affairs, can we allow the president enough flexibility to promote the national interest and still keep his vast powers limited and accountable, as constitutionalism requires? Koh and Henkin are troubled (Koh deeply, Henkin mildly) by evidence that the presidency has been able to evade both constitutional and statutory restraints in conducting the nation's foreign policy.

Henkin's book is strongest at what might almost be called the philosophical level. Constitutionalism, the United States' birthright, implies limited government. There can be no legitimate power that is "not checked, not balanced, not even the president, not even in foreign affairs" (p. 36). But if constitutionalism is our birthright, democracy has been our guest ever since the Declaration of Independence evoked the core values of equality and government by consent. Henkin stresses that a democratic constitution should promote the largest possible participation by the people and the most authentic representation—"open government . . . responding to the people's values" (p. 37). We are, he argues, a "dual democracy" (p. 40). The president commands information, expertise, secrecy, speed, and efficiency; Congress represents the people's "wider, more sober, more deliberate (more cautious) long-term values and judgments" (p. 43).

He admits to some unease about recent performance. In making treaties, for example, presidents should be more candid, the Senate more restrained (p. 68). In general, we should have "fewer decisions by one representative [the president] alone" (p. 43). Nevertheless, we do not need "amendment or radical reconstruction of the Constitution as regards foreign affairs in particular" (p. 40)—just a more assertive Congress and courts less prone to excuse themselves from "political questions."

Henkin has written many books on the Constitution and foreign affairs. He has earned the right to make a magisterial statement. Yet this is a disappointing book. Too often it is complacent where it needs to sound an alarm.

Koh's book is more substantial, fresher and more penetrating. Focusing on the Iran–Contra affair, it presents a comprehensive and devastating critique of the three major investigations: the Tower Commission, the congressional inquiries chaired by Senator Daniel Inouye and Representative Lee Hamilton, and the special prosecution by Lawrence Walsh. None of these efforts, Koh argues, came to grips with the fundamental issues: In an interbranch dispute over foreign policy, who decides? If constitutional and statutory norms are violated, how can the government be held accountable?

The Constitution, he notes, gives almost all the enumerated powers in foreign affairs (declaring war, raising and governing armed forces, appropriating funds, regulating foreign commerce, defining and punishing piracies and offenses against the law of nations) to Congress. Yet presidents "almost always win in foreign affairs" (p. 117). Why? The answer is that presidents conduct relations with foreign powers and have all the structural advantages (unity vs. bicamerality, concentration vs. dispersion of power). In addition, presidents lately have benefited from what Koh calls the "activist logic of this extrovert era" (p. 119).

On one key point, Koh and Henkin agree: Congress has the right, under the Constitution, to set limits on presidential foreign policy making. To establish this point, both rely on Justice Robert Jackson's concurring opinion in the steel seizure case of 1952 and set it against the expansive claims made by Justice George Sutherland in *United States* v. *Curtiss–Wright Export Corporation* (1936). Whereas Sutherland asserted that the president has powers in foreign relations that do not depend on the Constitution, Jackson argued that presidents who act in the absence of a congressional grant of power are operating in a "twilight zone," relying solely on their own independent powers. When a president acts against an explicit or implied congressional determination, as Truman did in seizing the mills (and as Reagan did in directing aid to the Contras in violation of the Boland amendments), "his power is at its lowest ebb," wrote Jackson (quoted on p. 108).

Koh concludes that Jackson's opinion establishes both the dominance of Congress over foreign policy making when it chooses to act and the responsibility of the courts to enforce this dominance when a case clearly presents this issue. The problem is that as a practical matter, Jackson's doctrine too often loses out to Sutherland's. Congress and the courts share responsibility for this weakening of constitutional bounds. Congress tends to acquiesce in presidential initiatives, due to "myopia" (tailoring remedies to the most recent abuses, rather than addressing the structural issues), poor draftsmanship (as in the War Powers Resolution), poor choice of legislative tools (reliance on legislative vetoes

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or a mustering of majorities to block presidential initiatives), or lack of political will. Why does Congress legislate this way? Koh draws on the work of Morris Fiorina and David Mayhew, among others, to explain how political incentives work.

What is the remedy? Koh prescribes new "framework" legislation and the appointment of a congressional legal adviser specifically for foreign affairs. We also need, he writes, a "constitutional substitute for the legislative veto" (p. 176), more effective use of the appropriations power, stiffer criminal penalties for violation of national security statutes, and application of the impeachment power against executive officials other than the president.

Having shown that the "National Security Constitution" laid down by the framers was ignored by George Washington and by every determined president since; that Congress has routinely acquiesced in these evasions; and that the courts, too, have been tolerant, Koh concludes that Congress and the courts must now insist on reaffirming it. Essentially, he would restore the rule of law by writing new laws and by implanting another lawyer in the system.

These professors of law agree that constitutional reform is unattainable and not worth serious consideration. Against current practices, Henkin invokes the norm of constitutionalism, while Koh calls for new organic statutes. Yet Koh, in particular, convincingly explains why these approaches have not worked since World War II. What changes their prospects now? These works, written by lawyers and informed by the best work in political science, leave us profoundly uncertain about the question whether we can give the president enough power to conduct effective foreign policy and still keep him accountable.

Smith College DONALD L. ROBINSON

The Poor in Court: The Legal Services Program and Supreme Court Decision Making. By Susan E. Lawrence. Princeton: Princeton University Press, 1990. 207p. $29.95.

It is all too rare to find a scholarly book that increases our understanding of institutional behavior, imparts new and interesting substantive information, and invokes innovative and appropriate methodology. *The Poor in Court* not only accomplishes these formidable goals but is also well written and meticulously researched.

From the opening chapter, we know that a good read is to follow, because Lawrence sets up an interesting research challenge. She seeks to explore two somewhat competing and certainly complex theories of Supreme Court decision making: (1) a large body of literature indicates that certain litigants are highly adept players in the legal game, that is, they have developed strategies and tactics that enable them to access the Court to achieve policy ends; (2) there exists an even larger stack of studies asserting that the primary determinants of judicial outcomes are the values and attitudes of the individual members of the Court. If the latter holds true, can litigants (particularly those of a different ideological stripe from the majority of justices) actually influence the process? Addressing this question (more aptly, *solving this puzzle*) is a difficult conceptual and analytical task, but one worth undertaking. Its solution is of obvious significance to those laboring in the field. More important, it would reveal much about the nature of Supreme Court decision making and the institution's role in the governmental process. Because Lawrence reaches a simple, yet credible solution—to which I shall return—this book expands and reconditions our understanding of the Court.

To begin puzzling together these approaches, Lawrence focuses on the litigation activities of the Legal Services Program (LSP). This was a good choice and, I think, an interesting one as well—*good* because the LSP did not possess the characteristics we often associate with successful litigating enterprises. Much scholarship on litigant behavior and influence has focused on interest groups, which often have clear-cut policy objectives and strategies for achieving those ends. For example, many are highly selective about the kinds of cases they enter. The LSP, though, was quite different. It had no grand litigation campaign in mind. It took clients "because they met the indigency requirements of the Program rather than because their cases fit into a litigation strategy" (p. 9). In a note, Lawrence reacts to criticism that she is biasing her study because others have indicated (as have statistical profiles) that the LSP was an influential player. I think she is correct. If a litigating operation, such as the LSP, can influence Court decisions, then we would have to reevaluate our existing knowledge. In short, the LSP was a good choice because it provided the ultimate test of the potential effect of litigants qua litigants. It also was an interesting choice. There are numerous examinations of the LSP—especially of its history and day-to-day activities—but not of its influence, assessed systematically. By exploring it through this lens, Lawrence imparts important information about the program and its ability to engage the poor in the civil litigation process.

The next task faced by Lawrence was to devise an appropriate research strategy to evaluate whether the LSP influenced Court decisions. Given the focus of her analysis, coupled with the many debates within the subfield of law, courts, and judicial process, this is a nontrivial issue, posing numerous challenges. First, she had to get a handle on the amorphous entity that is the LSP. Choices were called for, and she made reasonable ones. She limited the study to the LSP's litigation activities between the 1966 and 1974 terms (before it was officially replaced by the Legal Services Corporation) and then to those cases it appealed to the U.S. Supreme Court or in which it participated as an *amicus curiae*. Second, she needed to define *influence*. Though many of her predecessors operationalized that concept as gaining access to the Court *or* as convincing the justices to adopt arguments on the merits, she chose to look at both. Finally—and most critically—she had to devise a scheme for assessing influence, one that would satisfy all camps. Herein lies the volume's greatest strength: because she mixes quantitative and doctrinal approaches, she is able to speak authoritatively to the various constituencies and theoretical concerns of the subfield. Consider her *modus operandi* in studying the effect of the LSP on decisions on the merits. She initially presents compelling numerical evidence indicating that it was a far more influential player, across a diverse range of legal areas, than group or behavioral approaches would have predicted. She then presses the discussion to pursue the issue doctrinally, fully recognizing that wins and losses,

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for example, are one thing, the ability to shape the development of the law quite another.

This comprehensive, integrated approach pays off. When Lawrence argues that the LSP was extraordinarily influential, we believe her; the case is that strong. When she seeks to translate her findings into a more generalizable, theoretical framework, we also believe her; the argument is that appealing.

And what does Lawrence teach us about the puzzle of Supreme Court decision making? Most important is this: neither behavioral approaches nor those centering on litigant prowess provide a particularly compelling explanation of the LSP's remarkable success before the Court or of decision making. Instead, according to Lawrence, we must consider them in conjunction with "the temper of the times;" that is, the existing political climate can create "a 'window of opportunity' for the joining of new litigant claims, available legal bases, and judicial attention and sympathy" (p. 150). In terms of the focus of her study, such allowed the "poor's participation in Supreme Court decision making to produce significant doctrinal change." This conclusion is substantial, but its importance extends beyond the case at hand. Not only does it provide a reasonable solution to a long-standing research puzzle, revealing how those enmeshed in competing schools of thought can bridge their differences, it also has significant implications for our understanding of the Court and its role within the larger governmental process.

In the end, then, this book makes a major contribution to our understanding of things legal and extralegal. It is my sincere hope that all scholars of the judicial process read it. Indeed, my greatest fear about the book is its title. *The Poor in Court* is, yes, about the mobilization of rights for indigents, but it is about much, much more.

Washington University, St. Louis LEE EPSTEIN

Privacy in America: Is Your Private Life in the Public Eye? By David F. Linowes. Urbana: University of Illinois Press, 1989. 190p. $19.95.

Knowledge As Power: Political and Legal Control of Information. By David Sadofsky. New York: Greenwood, 1990. 133p. $38.95.

Sir Francis Bacon's aphorism that knowledge is power now has even greater relevance to the study of politics. First, there has been an extraordinary expansion in the creation and diffusion of knowledge, information, and data. Second, the widespread diffusion of the new information technologies, especially computers and telecommunications systems, has facilitated a major increase in the scale and sophistication with which humans can gather, manipulate, and transmit data.

For political scientists, it is particularly germane to consider whether this proliferation of information and information technologies has caused significant shifts in the distribution of power. We still do not have a well-developed theory of precisely how knowledge converts into political power. This area of inquiry includes many subtopics: Who controls contemporary information technologies and their products? Are data and information resources of power and influence similar to knowledge? Whose interests are served by the uses of these resources? Under what conditions are actors' rights violated by the uses of knowledge (information, data)?

David Linowes and David Sadofsky address some of these subtopics in their books. Each focuses primarily upon certain aspects of information use in the United States. Linowes is particularly concerned with issues of information privacy and considers whether the new information technologies have altered the relations between individual citizens and the large public and private sector actors who have access to information about those citizens. Sadofsky examines whether there are systematic differences in the information policies of different governmental institutions and whether the implementation of these policies affects inter-branch relations.

Linowes, who is a professor of political economy and public policy and served as chair of the U.S. Privacy Protection Commission in the late 1970s, mounts considerable evidence that widespread abuses of individual privacy are being perpetrated by institutional actors. In the journalistic tradition of David Burnham's *Rise of the Computer State* (1983), Linowes uses descriptive facts and numerous, detailed incidents to suggest a pattern of problems with the current collection and uses of personal information by government agencies and by such private organizations as insurance companies, banks, and credit-reporting firms.

"Vast amounts of personal information are being amassed without the subject's knowledge, and preserved indefinitely to be retrieved instantly. This information can be used in ways that have disturbing implications for an individual's personal freedom. The legal system," Linowes argues, "has lagged far behind new technological advances" (p. 9). He contends that many organizations gather or acquire information about individuals they should not possess and that in many instances they act upon inaccurate information. His examples suggest that neither individual action nor the system of statutory safeguards is sufficient to protect against widespread infringements on personal privacy.

Sadofsky, a joint public administration Ph.D. and J.D., grounds his analysis in contrasting views of the U.S. republic. He argues that a conception of strong executive governance, derived from the Hamiltonian perspective, has a corollary information policy that promotes the collection of vast amounts of information by the bureaucracy, the centralization of that information within the executive branch, and substantial constraints on public, and even legislative, access to that information. This conflicts with a Madisonian–Jeffersonian conception of participatory governance including concern for the protection of individual rights. According to Sadofsky, among these rights are the citizen's right to information about government decision and action and the protection of privacy, due process, and personal liberty by constraining bureaucratic uses of information. The Congress (delegated the task of informing the public) and, especially, the judiciary can act to limit the excessive information control attempted by the executive. But, Sadofsky argues, Congress has been too reticent in limiting the executive's information control and has failed to form its natural alliance with the judiciary in this policy domain.

Sadofsky's evidentiary base mixes consideration of the founding fathers' writings with both historical and contemporary court cases dealing with the government's right to limit information, the citizens' right to privacy, and the right of both to know. Sadofsky's own bias is clearly in favor of what he terms "robust openness of

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expression" and the widespread availability of relevant information during government policy debates. He opposes the information secrecy he attributes to "the hierarchical style of the executive" in its dealings with the other branches of government and the public.

There is less overlap than one might expect in these two short books. Linowes is concerned with the linkage between the individual citizen and large public and private organizations, especially the protection of individual privacy. Sadofsky examines the role of information in national, interbranch politics, especially interorganizational restraints on the free flow of information. Linowes's anecdotal style eschews theoretical grounding, systematic analysis and efforts at generalization. He also offers little political or institutional analysis regarding the structural relations among actors. Sadofsky provides some examples of information use and abuse but relies primarily upon on case law to support his premise about interbranch conflict over information policy. For Linowes, contemporary information issues are crucially tied to the impacts of the new information technologies. In his view, these new technologies have fundamentally transformed how information is gathered, manipulated, and used. One could read Sadofsky's book and not even realize that computers and information technology are now extensively utilized in government.

Linowes's book is perhaps most effective as a warning to the elite public and the policymaking community that the United States continues down the slippery slope of eroding personal privacy in the face of organizations' massive and constantly expanding uses of computerized data bases on individuals. It is less likely to appeal to political scientists interested in rigorous empirical analysis or theory development.

Sadofsky's book will be most appreciated by those interested in a well-argued critique—grounded in constitutional law—of executive constraints on the use of information in the policy process. It might be less satisfying for those who see the U.S. executive branch, judiciary, and national legislature as complex arenas of bureaucratic politics with extensive *intrabranch* differences as to why and how information is collected and used in the policy process, rather than as unitary actors driven by *interbranch* differences. One important consideration that seems underestimated in the discussion is the changing ideology of the judiciary (particularly the Supreme Court) regarding information policy issues. The absence of any explicit treatment of the effects of the new information technologies on information policy and use is also somewhat surprising. Overall, these books add to other research developing the basis for a richer theoretical and empirical explanation of the increasing role of knowledge (information, data) in the political process.

University of California, Irvine JAMES N. DANZIGER

Sex, Gender, and the Politics of ERA: A State and the Nation. By Donald G. Mathews and Jane Sherron De Hart. New York: Oxford University Press, 1990. 283p. $24.95.

Over the past decade social scientists studying the policy history of the Equal Rights Amendment (ERA) have seemingly turned on its head the old saying that victory has a hundred fathers and defeat is an orphan. The failure of this proposed constitutional amendment has been attributed variously to a limited window of opportunity, dynamics of community conflict, issue transformation typical of symbolic politics, inadequate state-based organizational resources and strategies, and shifting legal doctrines that undercut the urgency for policy adoption. Mathews and De Hart here reiterate, extend, and only rarely refute this previous scholarship. However, their conclusion that "primarily, ERA failed because a large number of men did not like being challenged by women, and they did not believe that the things women were interested in were important" (p. 211) barely hints at the theoretical richness of this most comprehensive examination of an unratifying state's (North Carolina's) struggle with ratification.

The authors base their analysis on participant observation, extensive archival research, and more than one hundred interviews, conducted from 1973 to 1987. North Carolina was considered one of the states most likely to approve the amendment before the deadline. And by the end of the 11-year period (1972–82) during which the ERA was before the legislature, the North Carolina ratification coalition had become perhaps the strongest in the nation.

Two themes are developed here. One involves the conventional history of state-level interest group competition and legislative decisionmaking, informed by discussions of the precedent of woman suffrage in North Carolina and the postsuffrage politics of the ERA (1923–72). The meticulous description can be both frustrating and fascinating. At times, the reader is sorely taxed to recall the official role of named individuals rivaling in number those in a Russian novel. To compensate, there are the rewards of scintillating "factoids" (e.g., the use of Sam Ervin's Senate franking privilege by anti-ERA leader Phyllis Schlafly and ratificationist mailings of chicken droppings to legislators opposing the ERA in the final vote of 1982.) This story is a familiar but engagingly narrated one.

The most original and important contribution stems from the authors' avowed purpose "not . . . to explain why ERA failed so much as what the debate surrounding the issue tells us about the women's movement and its place in this country during the 1970s. . . . The intricate and subtle fusion of sex, gender, race, and power . . . underlies resistance to change in 1990" (pp. xi–xii). In explicating the intersubjective meanings of the ERA for supporters, opponents, and legislators, the authors find a common ambivalence concerning the gender revolution of feminism.

Ratificationists emerge as far more committed to cultural and personal change through the ERA than they admitted publicly (or to themselves) in their appeals for women's rights, equality, and change in the name of tradition. For antiratificationists, the ERA was a feminist-led attack on the absolute rightness of traditional gender roles, the basis of personal identity, social legitimacy, and the moral order. Many legislators too saw the ERA as a symbol of feminist and federal intervention and associated it with a loss of power for men and state officials generally. In faithfully and sensitively giving voice to these perceptions, the authors neither give license nor abdicate their responsibility as scholars to point out inaccuracies and logical inconsistencies, provide interpretation, and draw implications.

Mathews and De Hart present a vivid portrait of apocalyptic politics in the United States. Here cosmopol-

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itan ratificationists, with their faith in social science, legal–rational authority, and bureaucratic forms of organization, discovered that knowing the conventional rules of pluralist politics and executing them well did not lead to victory. The paradigm of the United States as an underdeveloped nation receives much support here. But this is only one of several embedded texts, all of which are valuable and compelling for students of U.S. politics, public policy, interest groups, and gender.

Marquette University JANET K. BOLES

Getting Justice and Getting Even: Legal Consciousness among Working-class Americans. By Sally Engle Merry. Chicago: University of Chicago Press, 1990. 227p. $50.00 cloth, $19.95 paper.

Sally Engle Merry provides a thoughtful and rich ethnographic portrayal of a sample of white working-class individuals (primarily women) who seek redress for personal wrongs within the lower criminals courts and their associated mediation programs. This is a careful examination of citizens who bring their problems to the courts and of court officials who respond ambivalently, redefining the problems because they do not view them as truly legal issues.

Merry frames the interplay between plaintiffs and court officials through a discussion of three distinct discourses that coexist within the lower courts, each of which involves predominant categories and remedies: legal discourse that focuses on rights and evidence; moral discourse "[about] how people should treat one another, about relationships, and about respect and reputation"; and therapeutic discourse "[about] treatment and cure and about the way behavior is shaped by environment and social pressures" (p. 12). Plaintiffs tend to operate within a legal discourse, but court officials commonly redefine the problems and place them within moral and therapeutic discourses.

The author's insightful discussion of the citizens' sense of entitlement to seek court remedies discloses the tension inherent in this entitlement based in a broad sense of rights: that is, property, autonomy, and parental authority and the notion that court resolution of problems is more civilized and reasonable than violence. She provides instructive and lucid examples of specific types of problems and the distinct set of legal categories and rights associated with each: neighborhood problems (property ownership), marital problems (contract), boyfriend–girlfriend problems (protection against violence); and parent–child problems (state responsibility for teenage children in trouble). It is a complex portrait of individuals who do not express alienation from U.S. society yet evidence awareness of the social and economic inequality that eventuates their positions of relative powerlessness.

In this adept picture of contemporary citizens as clients, Merry suggests that due to the symbolic power in succeeding in producing a gesture from the courts, plaintiffs are empowered in relation to those against whom they bring complaints. Yet they are enmeshed simultaneously in the control of the courts and the concomitant power to define, reinterpret, and dominate. The risks of going to court are significant: stigmatization; having one's problems (especially in the cases of women) reinterpreted as issues of character and emotion rather than legal rights; and failure to find meaningful solutions.

The book is a strong contribution to the literature on mediation programs and dispute resolution, even as the author effectively points out that these individuals conceive of their complaints as "problems" rather than "disputes." Particularly important is her analysis of court processes as those of "cultural domination" in terms of the power of interpretation, categorization, and framing ways of talking and acting.

As is the fate of all subtitles (due to the requisite brevity), this volume's subtitle, *Legal Consciousness among Working-Class Americans*, is potentially misleading. This is not an encompassing look at the working class. It is a study of manifestations of the everyday consciousness of those who use the courts, that is, a relatively small proportion. It looks exclusively at whites (mostly women) in Massachusetts settings in the early 1980s. The author acknowledges the parameters of her analysis and includes a useful appendix summarizing other studies of court users that have demonstrated that "the social characteristics of court users are roughly the same as those I observed in Cambridge and Salem" (p. 183). Nevertheless, the use of the terms *consciousness* and *working class* engenders expectations of a macrostructural analysis of political economy, expectations that go primarily unfulfilled. *Consciousness* refers to the everyday thinking and reasoning of individuals, inferred by the author from informal conversations with them and observations of their "talking and behavior" in and out of court. Likewise, readers who expect an in-depth analysis of juvenile justice will find that it does not play a prominent role in the analysis. These limitations do not negate Merry's significant contribution; this thoughtful and illuminating book merits the wide audience and appreciation it will receive.

Arizona State University M. A. BORTNER

Governing the Commons: The Evolution of Institutions for Collective Action. By Elinor Ostrom. Cambridge: Cambridge University Press, 1990. 279p. $14.95 paper.

This is clearly an important book, but its significance depends upon how one interprets its intended scope. On the one hand, *Governing the Commons* can be seen as a major contribution to natural resource and economic development policy and, more specifically, to the management of common pool resources such as fisheries, groundwater basins, and irrigation systems. Such resources have long been subject to overexploitation and misuse by rational, self-interested actors. The conventional solutions are either management by an external governmental authority (Hardin) or privatization of the resource (Baden). Ostrom criticizes both approaches for their generally unrealistic assumptions (e.g., no communication among users of the resource) and instead explores the conditions under which a third alternative, the development of self-governing institutions by the resource users themselves, can be successful in achieving sustained use over considerable periods of time.

The broader—and, in my opinion, more correct—interpretation is that Ostrom's book is an example of an institutional rational choice analysis of solutions to problems of collective action. Here, again, the emphasis is on the development of self-governing institutions by the

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individuals directly involved; but the scope includes not only such relatively small organizations as law firms and cooperatives (p. 25) but also a wide range of situations subject to the underprovision of collective goods: "The central question in this study is how a group of principals [actors] who are in an interdependent situation can organize and govern themselves to obtain continuing joint benefits when all face temptations to free-ride, shirk, or otherwise act opportunistically" (p. 29).

Irrespective of one's interpretation, the book is clearly part of Elinor Ostrom's long-term research program seeking to foster an institutional rational choice approach to public policy. Comparing her analysis here with her earlier conceptual work (Kiser and Ostrom, 1982; Ostrom, 1986) reveals several continuities but also one important difference. The general conceptual structure remains very similar: she assumes that individuals are intentionally rational; that their behavior is a function partly of their own (self-interested) goals and information and partly of the decision situation in which they find themselves; that the decision situation is, in turn, a function of institutional rules and community characteristics (especially the extent of normative consensus); and that there are three levels of action, namely, constitutional, collective-choice, and operational—with outputs from higher levels setting the institutional rules at the subordinate level. Another recurring theme is the focus on institutional rules. In her discussion of the factors contributing to successful common pool arrangements, for example, she discusses community characteristics (discount rates and behavioral norms) in two paragraphs, while the analysis of institutional rules is accorded 12 pages.

While the general structure and focus is familiar, the discussion of institutional rules has been significantly revised. Largely absent is the abstract list of rules drawn from game-theoretic analyses of decision situations: boundary rules, scope rules, aggregation rules, payoff rules, communication rules, and so on. In its place is a much more "functional" analysis: How do the rules allocate access to the resource? How do they provide the infrastructure necessary to utilize the resource? How do they provide for monitoring of individuals' behavior, the imposition of sanctions, and opportunities to alter the rules? (pp. 42–45). The result is a set of eight "design principles" for successful self-governing institutions, including (1) clearly defined boundaries, (2) monitors who are either resource users or accountable to them, (3) graduated sanctions, and (4) mechanisms dominated by the users to resolve disputes and to alter the rules (p. 90). The basic task is to foster contingent self-commitment among the members: "I commit myself to follow the set of rules we have devised in all instances except dire emergencies if the rest of those affected make a similar commitment and act accordingly" (pp. 99–100). This is all very sensible; but people who have followed Ostrom's work would probably like an explanation for why so much of her previous set of rule categories has apparently been jettisoned. If they proved relatively useless, she should tell us.

For all of its strengths, this book still has some limitations. First, almost all the empirical work (except for the analysis of groundwater management in Southern California) involves secondary interpretation of research previously done by sociologists and anthropologists in specific local areas in Canada, Japan, the Phillipines, Sri Lanka, Switzerland, Turkey, and other places. In most cases, the results are drawn largely from the work of a single investigator, with no discussion of the investigator's methods or of possible problems with the validity or reliability of the results. The implicit rule seems to be *trust*—which for someone as methodologically sophisticated as Elinor Ostrom is most surprising. Second, Ostrom has self-consciously selected cases in which self-governing institutions should be relatively easy to study and *relatively* easy to organize (p. 26). Each involves a single country, a relatively small number of people (up to 15,000), and situations in which the individuals both heavily depend upon the common pool resource for their economic livelihood, and can harm each other but not outsiders (i.e., no significant externalities). In addition, her discussion in Chapter 5 strongly suggests that self-governing institutions are much more difficult to sustain in cases involving significant cultural and value differences or in which extralocal officials have some incentive to meddle in local affairs.

This book represents an "interim report" from one of the most sophisticated and important research programs in public policy. In their future work, I hope, Ostrom and her colleagues will give a little more attention to non-rule variables and explore the potential for dealing with collective choice problems in larger, more culturally diverse settings.

University of California, Davis PAUL A. SABATIER

Attack Politics: Strategy and Defense. By Michael Pfau and Henry C. Kenski. New York: Praeger, 1990. 191p. $42.95.

At the heart of Michael Pfau and Henry C. Kenski's project is the search for a viable response to negative political campaign messages. Because regulation of campaign discourse is constitutionally problematic, campaign self-regulation strategically unlikely, and news media exposure of unfair advertising improbable, negative advertising is here to stay. The remaining alternative, according to the authors, is the development of countermessages that "inoculate" supporters against the effects of attack messages.

Pfau and Kenski's suggested response to attack messages rests on the persuasion research begun by social psychologist William J. McGuire in the 1950s and 1960s and subsequently pursued by a number of others (well documented in the authors' bibliography). McGuire and his colleagues were interested in whether individuals could be made more resistant to persuasion, particularly under conditions of forced message exposure. The "brainwashing" of U.S. prisoners of war during the Korean War first drew McGuire's attention to the question.

Persuasion inoculation theory is really quite straightforward. Just as controlled exposure to an attenuated viral strain produces biological resistance to disease, controlled exposure to threatening attack messages encourages the stiffening of psychological resistance to persuasion. Inoculation theory posits that refutational pretreatments, usually offered in the form of a warning of an impending persuasive attack, "triggers the motivation to bolster attitudes, thus fostering resistance to counterpersuasion" (p. 75). Awareness of an impending

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attack message (threat) in conjunction with refutational preemption are both necessary for inoculation. Inoculation is particularly interesting as a potential counter to attack messages launched late in the campaign when the lack of time makes specific rebuttal messages impossible.

Pfau and Kenski tested the application of inoculation theory to campaigns in two experimental field investigations, one involving the 1986 U.S. Senate campaign in South Dakota and the other the 1988 presidential general election. In both studies the authors sampled potential voters in the Sioux Falls, South Dakota, metropolitan area (N = 733 for 1986, 314 for 1988). Subjects were randomly assigned to a variety of treatment conditions and a control condition. Using direct mail, subjects in the treatment conditions were exposed to *inoculations* offering either explicit rebuttal of the content of a subsequent attack message (*same pretreatment*) or a more generic defense of the candidate (*different pretreatment*), (p. 87). The 1988 field experiment included treatment groups exposed to either same or different inoculation treatments plus a treatment condition that offered a second reinforcement message prior to the attack message. Yet another group received only a postattack refutational message following the attack message.

Dependent variables consisted of receiver attitude toward the candidate or issue supported in the attack message and the likelihood of voting for the candidate supported in the attack message. ANOVA factorial analysis of variance was the primary statistical device used in the data analysis.

A complex array of findings is presented in the last 60 pages of the book. Generally, the authors demonstrate modest support for the inoculation hypothesis. Results suggest that inoculation pretreatments tend to (1) undermine the credibility of the source of the attack, (2) undermine the specific content of the attack, and (3) reduce the likelihood of successful persuasion regarding voting preferences. The difficulties with inoculation strategies, on the other hand, are (1) that achieving sufficient levels of threat from pretreatments, crucial for inoculation, in a campaign setting proved somewhat difficult, since in political campaigns subjects may tend to discount campaign messages of all sorts, including pretreatments (as the authors themselves conclude, p. 97) and (2) that the greatest success with inoculation is found among those who are already least likely to be susceptible to persuasion: strong party identifiers.

Though the authors are to be commended for imaginatively tackling what is perhaps the central problem of contemporary campaigns, the book is poorly executed. Over a third of the book is devoted to descriptions of various negative campaign messages, particularly since 1980. While offering little new information, these descriptions become tedious. It is not entirely clear why so much space was given merely to recording a widely recognized phenomenon, particularly at the expense of other substantive theoretical questions. The impact of negative campaigns on participation, for instance, is ignored.

The second part of the book is much more technical, with long passages of statistical information likely to lose undergraduates, many graduate students, and professors unfamiliar with factorial analysis of variance. The lack of a nontechnical description of methods exacerbates this problem. In short, while some readers may find themselves skimming the first part, others will have to struggle through the second part. These are unfortunate defects in an otherwise interesting application of theory to practice.

University of Utah STEVEN LIVINGSTON

Technologies without Boundaries. By Ithiel de Sola Pool. Cambridge: Harvard University Press 1990. 283p. $27.50.

When Ithiel de Sola Pool died in 1984, the world lost one of the great liberal optimists and a believer in the liberating power of instrumental rationality. Optimism and instrumental rationality are both much maligned these days, particularly within postmodern political theories. Thus, this book is unlikely to have the impact of his earlier, award-winning *Technologies of Freedom* (1983), to which, as the preface to the current volume points out, this book represents a sequel.

As was his method in *Technologies of Freedom*, here Pool's approach is to assess the impact of new communications technologies on existing and developing democracies. In his opinion, these technologies will soon rival in civilizing power the development of printing five centuries ago. Furthermore, the new "information society" is capable of reversing trends toward excessive centralization, urbanization, and nationalism that printing set in motion much to the detriment of Pool's overall goal, namely, the increase in individual liberty and opportunities for growth.

Throughout the book's wide-ranging discussion of satellites, computers, and digital transmission technologies, Pool iterates his canonical belief that the technologies themselves are ethically neutral with the antiphon that their results are invariably democratic unless tampered with by governments. This is the faith of Newton and his Enlightenment inheritors of modern science, a credo that gave birth to liberal governments as well as capitalist economies. There is a renewal of hope, as well, that technology might even return us to a more natural state (if not exactly the state of nature). Pool exclaims that a trait of the new technologies is a "return to more natural modes of communication . . . to overcome the artificiality in use or burdensome cost of media . . . to regain some measures of naturalness, spontaneity, interactivity, or ease of use" (p. 177).

Nowhere is this belief in the liberating aspects of communications technologies more evident that in Pool's discussion of development. While noting that "nature" respects no boundaries between states in making new satellite communication possible, Pool downplays the threat that such communicative imperialism (such as U.S. television) poses for cultural integrity within developing societies. Development in his view is always toward democracy, and communications technologies will aid in that movement if the "right-wing sentiment of protection of ethnic identity" is seen in its true light as "squarely against the liberal principle of freedom of information" (p. 115).

At the very least, Pool maintains, the technologies themselves are empty vessels, neutral carriers of the messages and substances entrusted to them. Thus, they reflect the nature of the societies they serve, whether authoritarian, oligarchic, or free (p. 195). But they hold great liberating promise for developing countries if those countries adopt the correct model for development. That

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is the "infrastructure approach," in which public funds are spent to provide necessary equipment for access to worldwide communications without regard to the substance carried over the air waves. Such an approach is "populist": "It defines development as the dispersion of autonomy and initiative throughout the society, and so it sees communications facilities as tools to be put in people's hands for them to use" (p. 183).

To his considerable credit, Pool's faith in technology and its rationalizing power never descended to the simplistic (and ultimately dehumanizing) dogma of waiting for the technological fix. He was far too insightful an observer of history and politics to mistake that instance of myopia for vision. Yet his "soft determinism" concerning communication technologies' ability to transcend political, as well as physical, boundaries ultimately ignores part of the essential nature of politics and rationality. Both manifest boundaries that it is not within their nature to cross without danger. Politics is about limited communities and shared opinions, not about universal humanity and ultimate truth. Technology itself cannot make human rationality capable of achieving either of these latter generalities. To assume it can is to mistake the nature of politics as somehow less than science as an essential human endeavor.

University of Connecticut RICHARD P. HISKES

Beyond 911: A New Era for Policing. By Malcolm K. Sparrow, Mark H. Moore, and David M. Kennedy. New York: Basic Books, 1990. 269p. $22.95.

Policing As Though People Matter. By Dorothy Guyot. Philadelphia: Temple University Press, 1991. 357p. $39.95.

These two books reflect a growing consensus in the research community that the "reform model" that has shaped U.S. police administration since the late 1920s has hit a dead end. At that time, top administrators faced organized bootleggers, meddling politicians, and a rank-and-file that was racist, corrupt, brutal, and lazy. The reform model stressed bureaucratic centralization, political independence, and internal inspections on the part of police organizations and a cool, detached professionalism (of a curious, by-the-book nature) on the part of individual officers. The daily activity of today's departments is workload- and technology-driven, pressured by 911 calls, dominated by radio dispatchers, and managed from a quantitative performance orientation that drives everyone to "make their stats."

Where has this gotten us? Now the police face soaring violence, drug-infested neighborhoods, crumbling housing projects, and shimmering racial tension. The stats have got worse. The police feel unappreciated, and the public feels ill-served. While this is not all their fault, what police have done to date has not gotten society out of this hole. However, the political strength of the myth of police as crime fighters has such a grip that it is a courageous leader who takes any but the easy course of just crying for "more police" in response. A few Jeremiahs can be heard, however; and ironically, many of them are to be found among the ranks—police chiefs and patrol officers who are aware that what they are doing is not working and needs fixing. Both of these books are organized around their particular police heroes and tell their tales of the new politics of policing in the context of local case studies.

Sparrow, Moore, and Kennedy have lots of heroes, from cities as diverse as London, Houston, Newport News, and Los Angeles. All of them have engaged in reshaping departments, moving them away from the reform model and toward a "community policing" orientation that stresses autonomy and personal involvement on the part of individual officers and decentralization and openness to outside influence by their organizations. From these experiences the authors have distilled a kind of primer for managing change in police organizations. The book details how their heroes confronted some well-known obstacles to police reform, including the macho values of the police subculture, bureaucratic rigidity, public indifference, and the risks of relying on the discretionary authority of street police (which are considerable and still include bias and corruption). It counsels that departments must open up so that the priorities of the communities they serve are reflected in policymaking and so that the public can learn more about the police and how they actually operate. The book describes how the complaints, community relations, and press operations of police departments currently serve just the opposite function. The authors also call for greater cooperation among the police, the public, and other public and private agencies. This is not trite; it is risky. The public often has things to say that the police do not want to hear. Other organizations have differing needs and priorities, and the police traditionally have not been very willing to accommodate them. Instead, they rail that no one else can understand them and that they could really be effective if everyone else would change. This book is based on site visits, interviews, and discussion of these issues by chiefs and policing scholars around a seminar table in Cambridge. We have to take the authors' word that the reforms were good or bad and succeeded or failed, but they make a convincing case. This is a wise and smart book, cast at about the level where both the researcher and the police intellectual will learn a great deal.

Guyot only has one hero, the reforming chief of upstate Troy, New York. He did not get as far as any of the others; it would be fairer to say that he moved his department *into* the reform period during the late 1970s and early 1980s, when Guyot was doing her fieldwork there. He tried to institute a few of the innovations described by Sparrow, Moore, and Kennedy; but they never got off the ground. This book has several strengths. It provides a great deal of detail about things that really matter in making organizational change, including how her chief dealt with a union, the civil service system, his city council, local newspapers, a fearful public, and zero budget increases. The detailed chapter on union power makes one wonder how heroes from the other cities got away with as much as they did. Union power is an important topic on which the Sparrow, Moore, and Kennedy are not particularly strong. General discussions are echoed throughout in narrative material gathered from the backseat of patrol cars. The Troy story is also interwoven with a review of past research on all of these topics and more, including a history of police reform efforts, the pathologies of police performance evaluation, the limits of official statistics, and the causes of crime. The result has a rambling, cut-and-paste flavor; but the literature review adds con-

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text and generality to the detailed case material that the other volume did not require. Actually, the books walk both sides of the level-of-detail question. While Guyot occasionally tells us somewhat more than we want to know about Troy, the Sparrow, Moore, and Kennedy volume is long on quotes but a bit short on evidence—it skips lightly over its case material. Their strategy works because it is deftly done, but in lesser hands the primer might not have been as convincing.

Northwestern University WESLEY G. SKOGAN

The American Revolution in the Law: Anglo–American Jurisprudence before John Marshall. By Shannon C. Stimson. Princeton: Princeton University Press, 1990. 228 p. $27.50.

Shannon T. Stimson's *American Revolution in the Law* presents a provocative and fresh historical account of the U.S. legal tradition. Focusing on the objectives of jural actors and seminal legal and political thinkers in England and America during their respective revolutionary upheavals in the seventeenth and eighteenth centuries, Stimson sets out "to challenge the assumption of a basic unity of vision and purpose at the roots of Anglo–American jurisprudence." In turn, she hopes to explain why "the question of the scope and limits of the judicial function in a democratic state is the overarching question of U.S. jurisprudence, whereas in English jurisprudence it remains a question of little significance" (p. 3).

Stimson disagrees with such "new historians" as John Reid and Thomas Grey, who discern a basic continuity between colonial legal assumptions and the common law tenets of Coke, Hampden, and Pym. She marshals evidence from important libel and treason trials and prominent political argument to conclude that the U.S. legal tradition departed substantially from England's in terms of such fundamental questions as the nature of sovereignty and law and the meaning and extent of citizen participation in such legal institutions as the jury. To be sure, English juries in the seventeenth century were compelled by circumstances (mainly the prevalence of uncertainty about the content of common law and concerns about despotic application of the law by judges) to develop a quasi-independent "nascent 'space' for judgment" (p. 4), which included jury nullification and some power to interpret the law. However, as the famous 1649 treason trial of the Leveller John Lilburne revealed, this expansion of the jury's deliberative power amounted neither to a radical change in the law nor to a norm of judicial independence. At the end of the eighteenth century, English juries still "accepted a version of law as essentially government's instrument, whose content they must rely on judges and magistrates generally to determine" (p. 30).

American legal change was more radical, pointing toward judicial review and a relatively independent judiciary. Drawing on Locke and the logic of the Enlightenment (Stimson's reliance on the influence of Locke's "framework of ideas" pits her against such historians as Bernard Bailyn and J. G. A. Pocock), colonial Americans developed a belief in democratic deliberation and a concomitant distrust of sovereignty, including judges appointed by the Crown. Legal actors in the eighteenth century viewed law as vital to the public interest of a democratic polity. As an essential domain of legal participation, the jury became the locus of a "widening [of] the space for reflective judgment about laws once made" (p. 59). Andrew Hamilton's appeals to the jury about "popular notions of government and society" in the famous 1734 Zenger libel trial epitomized this jury role. This expanded role of the jury not only established the conditions out of which the notion of an independent judiciary arose but also initiated the very tensions between unelected judges and the "people" that became an important feature of constitutional law.

Part 2 discusses some major perspectives on the nature of judicial power and review in revolutionary and post-revolutionary America, paying special attention to John Adams, Thomas Jefferson, Alexander Hamilton, James Wilson, and John Marshall. As is known, these men entertained different views as to how far government (especially national government) can be trusted and what relationships between nationalism and localism and between judges and "the people" (as interpreters of the law) were proper. Marshall tried to resolve these tensions by a jurisprudence that entailed "politics by discussion" (p. 139), but even he was unable to resolve the central tensions of judicial review in the U.S. democratic order.

Stimson's book is meticulous, thought-provoking, and largely persuasive. However, she could have demonstrated more clearly the links between the earlier theory and practice of juries and subsequent theories and debates about the power of judicial review. Though these institutions are linked by a common legal culture, the complexity of this culture (which Stimson amply demonstrates) makes the relation between jury practice and eventual judicial review unclear. In a similar vein, the connections between the views of judicial power discussed in Part 2 and the previous historical analysis of juries are not fully articulated or tied together. One also wonders how a consideration of James Madison's views would have affected the analysis and conclusions.

This said, Stimson's book provides an interesting and fresh approach to the emergence of debates over judicial review; anyone interested in constitutional history or theory must read it. Stimson impressively challenges major historians and raises points that are relevant to the virulent contemporary debate over constitutional interpretation. She does not address "how" to interpret the constitution; but she shows that the U.S. legal tradition provides no definitive answer to this question and that the practice of interpretation is inherently political and value-laden. This conclusion offers little comfort to those (like Judge Bork) whose critiques of judicial activism ultimately rest upon a separation of law and politics that is more reminiscent of Blackstone than the U.S. legal tradition Stimson so ably portrays. On the other hand, Stimson, like Sanford Levinson (*Constitutional Faith*, 1988) shows that debates over "who may interpret the constitution are also essential parts of our constitutional heritage. This suggests that critiques about the nondemocratic nature of judicial activism (including those of Edwin Meese and Bork) should not be automatically dismissed. The tensions between democracy and law are indelible parts of our legal tradition.

University of Wisconsin, Madison DONALD A. DOWNS

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State Supreme Courts in Nation and State. By G. Alan Tarr and Mary Cornelia Aldis Porter. New Haven: Yale University Press, 1988. 283p. $12.95 paper.

It has long been an accepted canon that state supreme courts can play an important policymaking role in their own state and in the nation at large. Unfortunately, few books have appeared that have sought to explain exactly what that role is. G. Alan Tarr and Mary Cornelia Aldis Porter successfully seek to enhance our understanding of these courts' policymaking role in this book. The central theme of the book, which recurs in all chapters, is that state supreme courts are in a position to carve out significant policymaking roles in the federal system. Moreover, they argue, the nature of that role will vary from state to state.

The authors develop these points in six chapters. The first two deal with relationships between state supreme courts and other actors in the federal system. In both chapters a significant amount of literature is ably digested and summarized. Not a great deal of new ground is covered, however. These first two chapters provide the basis for the meat of the book—case studies of change in three state supreme courts: Alabama, Ohio, and New Jersey.

Each state court is examined in detail in a chapter of some length. The approach used is not quantitative. Instead, the authors construct their case studies by relying on secondary sources, analysis of important cases, and (in the cases of Alabama and Ohio but, for reasons unexplained, not New Jersey) interviews with state supreme court justices.

Certain common issues are examined in each case study: the posture of each court prior to its change; the factors leading to change; and the current posture of each court in terms of such issues as relations with other state actors, relations with the U.S. Supreme Court, and each court's attitude toward innovations in certain areas of the common law. At the same time, details specific to each state are spelled out.

Finally, the authors offer some conclusions, of which two stand out. First, contrary to commonly accepted beliefs, rapid change can take place at the state level. Alabama, for instance, has moved out of the dark ages into the mainstream of U.S. law in a short period of time. What accounts for these changes? Here the case study method (especially one that uses a limited number of cases) may show its shortcomings. The authors point to the national court reform movement as one factor instrumental in fostering change. Other national trends (e.g., the Civil Rights movement) are also noted. Finally, in the case studies certain individual personalities loom very large (e.g., Arthur Vanderbilt in New Jersey), though their influence is downplayed somewhat in the concluding chapter. Indeed, one can come away from a reading of the case studies with the impression there is much that is idiosyncratic in the development of state courts. The examination of a greater number of cases may well uncover more general trends not easily found in a case study approach.

A second conclusion is that of diversity. Each court has an "institutional identity" that sets it apart from the others. Contextual factors, the authors argue, are important in establishing and fostering each court's identity. This raises a question that the authors do not—and perhaps cannot—answer fully. What contextual factors are important, and what is their relative weight? Here, too, the limitations of the case study approach may have appeared.

Thus, the book leaves a reader with many questions. For instance, what accounts for variation among state supreme courts? What is the relative importance of the different factors that foster change in these important policymaking institutions? However, no book answers all questions. In addition to showing the importance of state supreme courts in U.S. politics and enhancing our knowledge of their role, the authors have raised questions that should be of interest to other students of court policymaking and U.S. state politics. The book deserves a wide readership.

University of Wisconsin, Oshkosh DAVID M. JONES

Divided Democracy: Cooperation and Conflict between the President and Congress. Edited by James A. Thurber. Washington: Congressional Quarterly, 1991. 323p. $17.95 paper.

Congress, the President, and Public Policy. By Michael L. Mezey. Boulder: Westview, 1989. 255p. $15.95 paper.

Divided government, wherein one political party does not control the presidency and both houses of Congress, has increased since 1955. From 1955 to 1992 it prevailed during 26 years (68% of the time), compared to only 8 years (14% of the time) from 1897 to 1954. Two recent books do much to help us understand the nature, the causes, and the consequences of divided government, and include suggestions for remedying the current political gridlock between Congress and the presidency.

The Thurber volume is a superb reader containing 13 essays addressing important questions about cooperation and conflict between the Congress and the president. It is a very balanced work covering considerable ground, and it is designed to avoid presenting any single viewpoint or dominant ideology. Some of the contributors believe the system is working well, while others feel reform is needed. Views of both contemporary Madisonians and latter-day Hamiltonians are presented, while examining various arguments that ultimately center on efficiency versus representativeness and leadership versus democracy.

The range of topics is impressive. Thurber presents an overview of the roots of divided democracy and in a second essay examines the impact of budget reform on governance. Mezey contributes two articles, one that places Congress in the center of a presidential system and another that underscores the policy limitations of Congress. Insightful and somewhat contrasting views are presented by Pfiffner on the general problem of governance, Davidson on how divided government operated in three congressional eras, and Oleszek on the salient features of congressional policymaking in a divided government environment.

Other contributions focus on the congressional challenge to presidential regulatory relief (Foreman), war powers (separate pieces by Deering and Fisher), the removal power (Fisher), Congress and Nicaragua (Brenner and LeoGrande), and a macro transformational perspective (Dodd).

This is an excellent reader that merits adoption in a variety of courses. Some needs that should be addressed in the second edition are more material on the electoral

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origins of divided government (Jacobson's work), the role of the media in influencing both mass public and elite perceptions, the role of interest groups, and much more information on how political coalitions can be built to overcome stalemate. The latter topic is mentioned by Oleszek, but higher-caliber case study information along the lines of the Brenner and LeoGrande piece on Congress and Nicaragua would strengthen this very timely and important reader.

Mezey's work, by contrast, is a provocative, thoughtful, and well-written theme text that not only covers the origins, nature, and consequences of divided government but also proposes some broad solutions. Mezey is concerned that the status quo is all too frequently characterized by either stalemate or poor public policy. To build his case, he analyzes political institutions and public policy by offering some guidelines for both democratic and managerial policy criteria. He sees the current constitutional framework as a balanced system, compared to the representational system he favors for the United States. The latter is characterized "by a more dominant policymaking role for an executive operating through a political party or a coalition of political parties that commands a dependable majority in the legislature" (p. 15). He further believes that due to changing circumstances, we now have a constitution against government.

Mezey describes a transformed presidency and an evolved Congress, and follows with an analysis of the machinery of presidential domination. He emphasizes the failure of presidential domination, with either stalemate or bad policy outcomes. He finds the current system's efforts to address the problem as inadequate, including the use of commissions, automatic devices like Gramm–Rudman–Hollings and war powers, the legislative veto administrative strategies, and presidential imperialism.

Mezey's analysis is stimulating and informative, but his book falls short in the final chapter on what can be done to get the Congress and president to work together. Although his normative argument that we move toward a representational system is intriguing, there is little chance that Congress will give up its current policy turf to the president, even with Mezey's insistence that his representational Congress will still debate issues, exercise executive oversight, and provide service representation. Mezey's list of reforms is too broad and unrealistic, including public finance, higher legislative salaries, stronger legislative parties, and selective adoption of features of the parliamentary system like a mandatory question hour.

He emphasizes that "capitalism remains part of the problem" (p. 214) and that the system has too many independent actors but follows this with a generalized call for new arrangements to facilitate government action and a focus on reteaching the ideals of citizenship. He is also too idealistic in calling for candidates to cease using anti-governmental themes, thus pandering to the electorate's tax antipathy and to avoid using a mix of anti-tax rhetoric with spending promises to the audiences they are addressing. A presidency with a leading policy role of a more collective nature, supported by administrative resources and political resources of a reinvigorated national political party, is not likely to occur.

In short, there is a gap between Mezey's insightful analysis of the factors underlying divided government and his far-reaching solutions. If there is to be reform at all, it is more likely to be incremental and along the lines of Pfiffner's essay in the Thurber reader. Pfiffner suggests refining the checks and balances system a little by "continuing the use of the informal legislative veto, modifying the War Powers Resolution, and changing the rescission process—[to] provide for some accommodation of conflicting interests" (p. 57). Pfiffner also notes that there is no simple mechanical or process fix. In other words, "there is no substitute for leadership, trust, and comity" (p. 57).

University of Arizona HENRY C. KENSKI

The Expendable Future: U.S. Politics and the Protection of Biological Diversity. By Richard Tobin. Durham: Duke University Press, 1990. 313p. $45.00 cloth, $17.95 paper.

Declining biological diversity has been characterized as one of the most serious threats to global sustainability. Species are declining at such rapid rates that many are becoming extinct before they can be scientifically identified. In addition, the consequences of these extinctions are not readily apparent and are open to scientific conjecture. What *is* known, however, is that extinction is irreversible.

While at first glance, declining biological diversity may appear as strictly an ecological problem, Tobin accurately portrays the issue as distinctly political. Although the loss of species is a worldwide matter, Tobin limits the scope of his investigation to the United States' efforts to curb increasing extinctions, citing the lead role in protection of biodiversity taken by the United States. Tracing the history of the Endangered Species Act and its amendments, Tobin forcefully argues that decisions are based as much on political motives and realities as upon biological data.

Rather than merely presenting a legislative history, Tobin reports efforts to preserve biological diversity as dynamic and changing. As priorities evolve and administrations change, the tactics and procedures employed by federal agencies and Congress also shift. Tobin examines this dynamic component through in-depth analysis of the Fish and Wildlife Service's ability to act effectively in reducing extinctions caused by people. In addition to simply assessing the agency's administrative power, Tobin presents detailed studies of the listing process, designation of critical habitats and development and implementation of recovery plans. Again stressing the political component in each, he asserts that as the costs (real or imagined) are recognized, the agency's ability to act is constrained either by Congress, more powerful agencies, or both. Furthermore, the service has encountered so few successes that it often acts to avoid conflict, preferring to compromise with other federal and even state agencies. These political maneuverings often result in a less-than-desirable situation for the species in jeopardy.

To complement his examination of the processes involved in implementing the Act, Tobin presents several useful case studies. Although most are no more than a few pages, these examples are especially valuable because they illustrate the political nature of preventing extinctions caused by people.

While Tobin's presentation of declining biological diversity as a political issue and his thorough analysis of

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the Endangered Species Act are quite well done, *The Expendable Future* is not without flaws. For instance, Tobin's lack of assertiveness in his final assessment of efforts to preserve endangered species is especially glaring. Despite his statements that the program has generated few (if any) real successes (p. 254), Tobin asserts "depending on one's hopes and expectations, efforts to protect endangered species in the United States are either a modest success or a massive failure" (p. 257). Although the formal mechanisms for protection may have prevented some species from reaching extinction, several protected species have become extinct; and of the few that have recovered, most have done so independently of federal efforts. To regard the act as even a modest success seems too generous.

The low emphasis placed on prescriptions to improve the preservation of biological diversity is also disappointing. Tobin does have some valid suggestions for improving the overall effectiveness of the act; but these recommendations, like his conclusions, are quite timid. Tobin devotes only two pages to his six proposals, geared mostly to improving implementation of the Act. As is evident in his analysis of the Act, better implementation may not be enough to guarantee protection of imperiled species. Although Tobin does move in the necessary direction with two of his proposals (shifting of the burden of proof and the creation of a new agency for the preservation of biological diversity), I would have liked to have seen some more innovative solutions.

A final troubling aspect of the study is Tobin's avoidance of the larger issue of sustainability. Of key importance in this larger issue is the preservation of ecosystems as effectively functioning units. Vital habitat is also essential to the preservation of biological diversity. The linkages between sustainability of ecosystems and the maintenance of species diversity are quite strong. With the increased emphasis the social sciences have given sustainability in recent years, it is truly disappointing that the concept of sustainability is not incorporated into *The Expendable Future*.

These reservations aside, Tobin has contributed valuable work to the understanding of the regulatory process. Not only does he provide a thorough account of efforts to preserve endangered species, but his work provides excellent analysis of the actions and interactions of Congress and the various agencies. Through Tobin's constant attention to framing the protection of endangered species as a political issue, the reader is able to gain insight into important factors and dynamic processes involved in obtaining social goals.

University of Arizona DAVID COLNIC

The Decline of American Political Parties: 1952–1988, By Martin P. Wattenberg. Cambridge: Harvard University Press, 1990. 203p. $10.95 paper.

The Demobilization of American Voters: A Comprehensive Theory of Voter Turnout. By Michael J. Avey. New York: Greenwood, 1989. 152p. $37.95.

Despite being in the same general area and dealing with related—and at times overlapping—concerns, the conceptualizations, the use of data and supporting materials, the arguments being made, and the intent of these two volumes are more notable for their dissimilarities.

Martin P. Wattenberg has supplemented the earlier edition of his influential and well-received *Decline of American Political Parties* by adding a chapter analyzing the 1988 and 1984 elections within the overall context of the book's thesis. Wattenberg's argument, based principally on survey data from the National Election Studies, is that the declining association with the political parties and their decreasing impact on the vote is a result of a "a growing neutrality in the electorate's perceptions of the parties" (p. 66). An alternative explanation that negative feelings fuel the public's increasing distance from the parties is rejected. The consequences of the trends developed are that political parties

> are considered less relevant in solving the most important domestic and foreign policy issues of the day. In the voters' minds, the parties are losing their association with the candidates and the issues that the candidates claim to stand for. . . . Major new issues that arise will probably not help the parties rebuild their base of support unless voters are convinced that the parties can serve a meaningful function that candidates alone cannot. (p. 89)

Do the elections of 1984 and 1988 support this evaluation? Basically, yes.

Wattenberg argues that a quiet revolution occurred during the 1980s, due in large part to clear—even dramatic—policy differences between the two parties, the ability of voters to apply retrospective judgments of direct relevance in the elections, and the party-building efforts of Ronald Reagan. Republican party identification increased, Democratic identifiers continued to decline, and the margin of difference in partisan identification between the parties narrowed to its lowest level since the first evaluations in 1952. (Although the Democrats continued to maintain an edge among likely voters, this was reduced to roughly one percentage point, again a Democratic low point and Republican high point for the period covered.) For the first time also, Independents outnumbered Democrats. One consequence is that the Republican presidential candidate can concentrate his appeal on the party's core, making his or her election less dependent on attracting independents or Democrats. The proportion of George Bush's total vote from Republican party members was the highest recorded since the introduction of the party identification measures.

Nonetheless, such a realignment has not led to revitalized partisan identifications. The shifts have not penetrated to the congressional level. Split-ticket voting continues, as does rule by divided government: "Such a realignment is hollow when the two parties continue to have a weak image in the public mind and an uncertain role with future of American government. Realignment has thus been muted in the candidate-centered age in comparison to the electoral upheavals of the past" (p. 135). The decline of partisanship has not been reversed. The public continues to experience difficulty in relating the parties to the work of government and in contributing to the resolution of the most pressing of the society's problems. It is a bold argument. It should again win friends for the book and add to its reputation as one of the most original of contributions to the thinking in the area.

Michael J. Avey has written quite a different book. Avey develops a theory of voter turnout meant to be richer and more comprehensive than any presently available. He places emphasis on the role of political elites and policy choice in the mobilization and demobi-

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lization of electorates as being more relevant as keys to voter involvement than demographic indicators. His argument is that the parties do not adequately represent the interests of the working class and the poor in particular. If one party (or both) did, participation could rise to 80% of those eligible:

> The strongest motivation to vote is seeing concrete benefits resulting from elections. . . . mobilization involves providing or promising benefits. Demobilization is accomplished by increasing costs or decreasing benefits. The focus of analysis in turnout should be the intentions of political elites to bring people to the polls or keep them at home. The political elites control both the costs and benefits of voting. Successful political mobilization appeals to the interest of the target population, makes the participation of the target populations easier, and empowers the participation of that population. (pp. 27–28).

Avey then refines and applies his theory primarily within the limits of secondary source materials available. The canvass is broad and the objective ambitious.

There are problems with the approach, however. The model itself is general and nonpredictive, more of a synthesizing perspective than anything else. The inclusion of the findings of such a range of studies can result, on occasion, in a terse and choppy presentation. A more elaborated and fluid development might help the reader and give the book a broader audience. The secondary research used to flesh out the model more often than not employs a limited time frame or a narrowly prescribed focus or (alternatively and less frequently) a broad, encompassing categorical overview. Each presents a problem in integration. In addition, the author may take the socieconomic model too literally as the principal alternative target for attack. As I see it, it illustrates empirical associations and tendencies, some more debatable than others (e.g., the relationship between an increase in formal education and turnout or what such a variable may provide a proxy for). It is not the most flexible or inclusive of approaches nor the ultimate explanation for the range of factors that influence participation. It is, however, an empirically well-developed mapping of socieconomic status conditions associated with voter involvement.

The author's broader conception does perform a service by putting politics back into the equation; and his emphasis, much like Wattenberg's, on the relevance of policy stands in attracting voters is useful. Also, his wide-ranging knowledge of the appropriate social science research (in particular, the variety of historical studies called upon) provides a useful point of reference for others in the area. A good deal of effort, hard work, and intellectual energy has gone into the construction of this analysis.

In sum, then, the two books have different strengths. Both address questions of significance (each in its own way); and while the audiences and uses may well be different, both deserve attention.

Northwestern University WILLIAM CROTTY

Interpreting the Constitution: The Supreme Court and the Process of Adjudication. By Harry H. Wellington. New Haven: Yale University Press, 1990. 196p. $22.50.

The issue for Wellington is how to reconcile the Supreme Court's power of judicial review with representative democracy: "One difficult question that emerges [from a review of the Court's history] is how we in a democracy can tolerate regulation without representation" (p. ix). Wellington's answer is a blend of ascertainable common law adjudication and pluralistic politics. The Supreme Court's particular view of the Constitution will and should prevail, he writes, "when it is proven politically digestible." What make it digestible "turns in part on the Court's prior interpretation" (p. 158).

Of course, in arriving at this conclusion, Wellington adeptly dissects other attempts to reconcile democracy and judicial review. For example, he argues that Robert Bork and the originalists–intentionalists' search for interpretive authority is in vain. The Constitution was a compromise among conflicting intentions—the results of politics, not consensus. To find a clear intention in that result is illusionary. Equally troublesome as a source of high court authority are the contentions of the participation interpretists such as Justice Stone in *Caroline Products* or John Hart Ely. The robust judicial review needed to protect the democratic process and keep participation open is not as neutral as the participation interpretists have asserted. To identify a participation malfunction and (certainly) to cure that malfunction necessitate value judgments. Indeed, it is not uncommon to see a correct process leading to injustice. The Frankfurter–Thayer judicial restraintists fare no better in Wellington's view. They simply put too much faith in the Legislature's loyalty to the Constitution.

Wellington is inevitably led to the common law approach to constitutional interpretation to enhance the high court's authority and at the same time to impose sufficient restraint. In line with Alexander Bickel, Wellington contends that the Supreme Court's legitimacy will come only from the purity of its deliberative process. The Court must go about deciding cases in its own fashion distinct from executive or legislative decisional methods. Thus, authority will surround the justices' constitutional interpretations. Further, as an integral part of the high court's decisional sequence, the adversary process will provide the nexus between law and politics. To succeed at the Supreme Court level, attorneys must argue that their clients' interests are the public's interests. The interaction among the justices as they weigh the interests urged upon them by the attorneys constitutes the necessary negotiations as to which claim best coincides with the public interest. Of course, much is at stake. High court decisions still possess a serious finality; and, of course, they have immediate consequences. The losers pay a price.

However, the validity of Wellington's common law argument rests on the "digestibility" of the Court's rulings. Thus, the important ingredient becomes public morality, or what might be called *pluralistic politics*. The Court must weigh the consequences of its constitutional interpretation against a background of political noise (e.g., public protest, legislative response, media coverage, academic reaction, group activity) and within the confines of *stare decisis*. Continuity in the law is provided by *stare decisis*: "Like cases should be treated alike" (p. 12). Change is prompted by the political background noise. To illustrate the feasibility of his approach, Wellington skillfully analyzes the current abortion controversy. By tapping public morality the difficulties the Court has had in reconciling *Griswold*, *Roe*, *Stanley*, and

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Webster would have been eased, and the results would have been more digestible.

This common law method of judicial review "best explains American constitutional law, and, . . . while far from perfect, it is normatively superior to rival approaches" (p. 78). Perhaps. Wellington assumes that there is a clear and discernible U.S. morality or a set of common values and attitudes that lawyers can argue and isolate but observant justices can identify: "For the Court and the lawyers who argue before it, reasoning from these commonly held attitudes should be an important method for interpreting the values—the public morality—that are a source of law" (p. 107). Further, he assumes that the path of the law through *stare decisis* is, indeed, straight and narrow, although often uphill without switchbacks, detours, or alternate routes. Also, he assumes that the justices by training and inclination are objective decision-makers without agendas who are able dispassionately to weigh competing versions of public morality. Wellington is either overly optimistic or politically naive.

Although the conversational style of the book and the sequence of chapters sometimes distracts, Wellington's ideas remain provocative and his solution worthy of contemplation. Except for his argument that the Court's authority should rest on its unique decisional process, I remain skeptical of his solution to the incongruency between the Court as constitutional interpreter and representative democracy. I am not convinced that the Court's common law results will be superior to, or more digestible than, what the so-called originalists, textualists, or structuralists might devise. Nonetheless, his book is a good read and should not be ignored by those of us concerned with the threat of "government by the judiciary."

Washington State University CHARLES H. SHELDON

Mismanaging America: The Rise of the Anti-Analytic Presidency. By Walter Williams. Lawrence: University Press of Kansas, 1990. 179p. $19.95.

Walter Williams, a former policy analyst in the Office of Economic Opportunity, has written this book in anger and frustration. His central theme is that the critically important need for expert policy information, analysis, and advice at the highest levels of U.S. government has been countered by a deleterious decline in institutional analytic capacity throughout the executive branch and a serious weakening of the policy management capability of modern presidents. Both of these developments, he argues, have contributed directly to inept presidential governance and unsound public policy. Williams is quite emphatic in his view that the executive branch's analytic and managerial structure is in a serious state of disrepair and badly in need of a major institutional fix.

Williams firmly attributes the decline in the analytic capacity of the executive branch to Ronald Reagan, the first modern anti-analytic president, who launched a successful eight-year war on policy information and analysis and destroyed the capacity of White House analytic staffs to serve the institutional presidency. Faulty policy analysis, Williams argues, led directly to the massive budget and trade deficits during the Reagan years, lost opportunities in education policy, and the saving and loans scandal. Williams is equally firm in dating the decline in managerial capability from the end of the Eisenhower administration and claims that organizational competence has now become the pivotal missing ingredient in the modern presidency.

The institutional fix proposed by Williams is not dissimilar to what the Brownlow Report attempted to do more than 50 years ago and what various post-Watergate reformers have been advocating for more than a decade now: that is, redefine the proper role of the presidential staff in the context of an organizational theory designed to achieve a reasonable balance between neutral competence and responsive competence in the Executive Office of the President (EOP). A major part of Williams's fix is his plea to restore a strong policy analysis capacity to the White House; and, while fully recognizing that policy analysis can be abused, overused, and fallible, he argues fervently that high-quality policy analysis does facilitate effective decision-making and is a necessary condition for the policy changes needed to combat current economic and social problems. Williams's prescriptive approach to the problem of presidential staffing is a welcome contribution because of the central focus on policy analysis in his design for a new-look EOP; and that is what makes this book distinctly different from, and more interesting than, other post-Watergate prescriptions for change. It introduces policy analysis as an significant but hitherto ignored variable in the debate about presidential staffing and ensures that it will be ignored no longer.

Williams's other principal concern—the lack of organizational and managerial mastery in the White House—is heavily dependent on Eisenhower revisionism. Eisenhower was an organizational virtuoso, according to Williams; and things have not been the same since. Contrary to the views of Stephen Hess and others, Williams argues that presidents *must* be managers but that Eisenhower was followed by a succession of anti-bureaucratic (Kennedy and Johnson) or antigovernment (Nixon, Carter, and Reagan) presidents who lacked in-depth experience with large-scale public organizations and undercut the organization and management capability of the executive branch by politicizing and centralizing policymaking power in the White House. It is, however, hard to reconcile the author's criticism of post-Eisenhower presidents with his admiration for Eisenhower's managerial skills. It was, after all, Eisenhower who began the politicization of the EOP by being the first president to appoint non-career heads to the Bureau of the Budget and National Security Council staff and Eisenhower, again, who showed his successors how much scope presidents had to expand the presidential staff. While Williams extols Eisenhower's "hidden-hand leadership," he does tend to ignore Eisenhower's hidden-hand staffing practices that pointed the way to the assault on the doctrine of neutral competence in later decades.

In some respects, the excesses that often accompany historical revisionism get in the way of Williams's thesis. His perspective on Eisenhower's organizational virtuosity is open to question, as are the numerous generalizations about the damage done by Kennedy's organizational leadership. In particular, the author's assertion that Kennedy mounted "a full, frontal attack on the presidency and on institutional commitment" (p. 112) is singularly unpersuasive.

Notwithstanding controversies over presidential revisionism, this book has much to commend it; and if one

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accepts Williams's optimism about the possibilities of reform, then his "guiding propositions" (chap. 7) for structuring and staffing the EOP represent the most detailed and thoughtful blueprint for the role and function of the presidential staff since Brownlow. Adherents of what Williams calls "the superrealistic approach" to presidential staffing are, however, unlikely to share that optimism and will probably remain convinced that responsive competence has a much tighter grip on the nature of the U.S. presidency than Williams is willing to credit.

Australian National University JOHN HART

FDR and the News Media. By Betty Houchin Winfield. Urbana: University of Illinois Press, 1990. 277p. $34.95.

Those of us who thought we had learned all we needed to know about FDR and the media from studies by Graham J. White and by Richard W. Steele (and earlier from the research of Elmer Cornwell and others) will find fresh insights and data in this work. A professor in the School of Journalism at the University of Missouri, Columbia, Betty Houchin Winfield deals with the subject in broad compass but is adept also at analyzing Roosevelt's flair for dealing with radio and photograph opportunities, as well as with journalists.

Much has been made of FDR's charming manner and buoyant voice in his fireside chats, his skill at repartee in his press conferences, his mastery of the campaign speech. But along with all this, there was in the White House an extraordinary amount of careful planning, attention to detail, and sheer professionalism. Roosevelt had the help not only of the expertise of veteran journalists like Steve Early, the communications talents of Eleanor Roosevelt, and the writing skills of brilliant ghostwriters like Samuel Rosenman and Robert Sherwood. He enormously expanded the media impact of the White House through enlarged and invigorated techniques for reaching the populace in general and special targets in particular.

Indeed, Winfield, like a good political scientist, emphasizes this system rather than merely the impact of one man. Even more, she pictures graphically the overall context in which the Roosevelt administration operated—from the early emergency days, to the radicalized political environment of 1935–36, to the political and institutional difficulties FDR encountered during his second term, and finally to the whole new environment of world war. It was the ability of FDR and his White House to adapt to the momentous changes around them that account for his ultimate effectiveness both as Dr. New Deal and Dr. Win-the-War.

But it is also this expanded system and FDR's manipulation of it that worry Winfield as a believer in a truly free press. Roosevelt was a master of withholding information, as well as providing it; setting his own timing; playing favorites with friendly reporters while promising not to do so. He wanted to make the White House reporters his collaborators. When some correspondents rejected the enticing embrace, he treated a few of them as enemies. With his extensive network of personal friendships and the expanded influence of the White House, he could reach into the interstices of press and radio, exploiting rivalries, rewarding friends, thwarting adversaries.

Roosevelt was not loath to take on barons of opinion opposed to the New Deal, such as William Randolph Hearst, Colonel Robert McCormick, and a galaxy of other publishers. More than any president before or after, he attacked the concentration of control within the media—especially after publishers like Hearst began to buy up radio stations. When newspaper magnates charged the New Deal agencies with imperiling freedom of the press, he answered that their monopolistic power was the real threat. Thus, he triggered a great debate that sadly has not been continued in recent times.

The author ends up rather ambivalent (or should one say *fair-minded*?) in her overall estimate of FDR's impact on the media during the New Deal years. On the one hand, he provided massive amounts of information about New Deal programs, guided reporters through the complexities of novel policies, brilliantly explained his programs to the public through intimate chats and fighting campaign speeches. He was a great teaching president—the best kind of leadership. On the other hand, there was that "system" that enveloped the reporters, tended toward indoctrination, inhibited the press as "watchdog."

Perhaps a good balance was set in the New Deal years between these two tendencies of teaching and control. World War II, however, upset that balance, enormously magnifying administration control of the press, sharpening the question—as we proceed to enter more wars—of wartime controls at the hand of less benign and less democratically minded chief executives than Roosevelt. To her study of both the New Deal and war years Winfield has brought exacting scholarship, a sophisticated eye, and a penetrating understanding of the implications of news management for democracies, especially during wartime.

Williams College JAMES MACGREGOR BURNS
University of Richmond

City Choices: Education and Housing. By Kenneth K. Wong. Albany: State University of New York Press, 1990. 218p. $44.50 cloth, $14.95 paper.

A contribution to the study of urban politics and public policy that addresses a theoretical question, this book also presents a logical problem. The question is whether recent policy decisions in Milwaukee and Baltimore demonstrate the importance of politics over economics. In particular, the author poses a "political choice" model to subsume an "economic constraint" model derived, he says, from the work of economists and political scientists such as Paul Peterson (*City Limits*, 1981). The evidence developed to answer the question consists of case studies of the implementation of federal programs in the two cities. He defines two of the case studies as illustrations of redistributive issues (compensatory education and rent subsidies) and two as growth issues (vocational education and community development block grants). As one might expect, the author answers his central question in the affirmative.

Policy options from a political economy perspective increasingly revolve around the central trade-off between redistribution for the poor and economic development. Political systems as different as the emerging

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nations in Eastern Europe and financially hard-pressed cities in the United States all face that choice. In cities like Baltimore and Milwaukee, local officials need investment and growth in order to reverse the decline in their economies. To them, redistribution looks like increased costs to industry leading to a foregoing of investment while dividing a decreasing pie. Against that view are the political and moral appeals of redistribution.

Wong says that the previous literature now raises "the economic constraint" as the imperative: city officials must go for development. If that is true, where does politics come into play? The author finds that one type of political process, *clientele participation*, did not work because the mobilization of African-American interests in the two cities did not have an effect. Another political factor, *institutional diversity*, did.

The impact of institutional diversity provides the crucial evidence for Wong's thesis. To find whether political factors do more than explanations based upon economic imperatives, we must look to evidence in situations where the two factors predict different outcomes. Cutting his analysis to the core, this evidence occurs in federal programs fostering redistribution in the cities. Here the economic constraint model predicts program stability or decline, whereas the political choice model predicts at least slight program growth. Wong finds that local officials implement redistribution programs in education (program growth), despite the economic factors, because of their political independence and professional values—the institutional diversity factor. To his credit, Wong recognizes a similar finding in the prior literature (J. David Greestone and Paul Peterson's *Race and Authority in Urban Politics*, 1976). Based on this finding, the author poses an integration of political and economic factors in his political choice model.

Logically, federal funds serve to relax economic constraints at the local level. They should ease the difficulty with the development–redistribution trade-off. Consequently, issues of expending federal funds may not present the best test of the limits of the economic constraint model. If the economic imperative is so overwhelming that it leads to a violation of federal guidelines, then the economic constraint model may be truly predictive. Alternatively, if our conclusion is that politics play a role only when resources come from exogenous sources, the influence of local politics may, indeed, be limited. Most scholars who understand economics do not doubt the importance of politics in, for example, establishing the regime that structures economic relations. Perhaps the problem is with the common definition of *politics*.

Wong has achieved a good discussion of important policy choices in two interesting cities and placed his discussion in a theoretical context. This book will be a useful addition to reading lists for courses in urban politics, local government, and public policy.

Florida State University CHARLES F. CNUDDE

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COMPARATIVE POLITICS

Computers Inc.: Japan's Challenge to IBM. By Marie Anchordoguy. Cambridge: Harvard University Press, 1989. 258p. $25.00.

Marie Anchordoguy's *Computers Inc.* provides a bird's-eye view of the workings of the Japanese political economy by examining one of the vanguards of the Japanese success story, its computer industry. Focus is given to the myriad government industrial policies in support of computers, including import and foreign investment controls, subsidies, low-interest loans and loan guarantees, tax incentives, the promotion of cartels and industrial concentration, and cooperative research and development projects.

Special attention is drawn to one of the often-overlooked ingredients in this success story, the Japan Electronic Computer Company (JECC). The JECC would purchase hardware from computer makers and then rent it out to users at rates 40% below that of the domestic makers' major competitor, IBM. The JECC would provide secure markets for the manufacturers, boost their market share, and allow them to move down learning curves and gain vital scale economies. In the 1960s, the JECC was responsible for 65% of all Japanese computer rentals.

This well-informed, well-researched volume draws much of its empirical richness from indispensable interviews with government and industry officials. Anchordoguy also has a flair for revealing summary statistics. For instance, it is calculated that in the 1960s, Japanese government subsidies and tax breaks amounted to 46% of the industry's investment in research and development and plant and equipment; in the early 1970s, the figure rose to 57%.

Anchordoguy's primary objective is to describe the actions and policies of the JECC, the Ministry of International Trade and Industry, and Nippon Telephone and Telegraph, as well as such computer makers as Fujitsu, Hitachi, and NEC. The impressive advances made by the industry are then described. While strong on description, this work does not offer rigorous explanation. Industrial advances are not closely tied to specific actions or policies (admittedly a difficult task). No attempt is made to weigh the respective contributions of the different governmental and industrial actors. In fact, this work specifically steers away from such explanatory tasks. This is unfortunate, since much of the debate over the Japanese political economy revolves around the relative influences of statist, corporatist, Marxist, and market factors. It is also unfortunate because Anchordoguy's research contains the raw material for testing theory.

This volume takes note of numerous shifts and changes in Japanese policy toward computers. But Anchordoguy does not go sufficiently beyond her bird's-eye view of the industry to take full stock of these changes. Her own data indicate the momentousness of these changes. The figure for government subsidies and tax breaks compared to private investment in research and development and plant and equipment dropped to 25% in the late 1970s. And JECC's share of computer rentals plummeted to 11% in the early 1980s. Now, more than ever before, Japanese studies must appreciate the dynamism of the country's political economy. Insight must be provided not just into where the country is coming from, but into where it's headed.

University of Illinois, Chicago GLENN R. FONG

The State and the Industrialization Crisis in Turkey. By Henri J. Barkey. Boulder: Westview, 1990. 220p. $28.50.

It has not been very long since political scientists began to demonstrate renewed interest in the so-called field of political economy. Studies of the formulation and the consequences of economic policies, and of the interaction between the political and the economic processes of society, and the outcomes they produce, by using techniques of structural analysis, policy analysis, and economic analysis, constitute one manifestation of this interest. Henri J. Barkey's examination of Turkey's industrialization process falls within the growing tradition of such studies.

Barkey begins his analysis with a review of the evolution of import-substituting industrialization (ISI), which has been characteristic of countries that have chosen to industrialize during the twentieth century. Whereas it has proven relatively easy, he argues, to generate support for the adoption of a policy of import substitution, the policy itself seems to run out of steam and exhaust itself after a while. This exhaustion "has been credited with worsening economic and concomitant political crises that ultimately led to the praetorianization of society and the unraveling of many countries' political systems" (p. 14).

Barkey challenges the conventional wisdom that the ISI paradigm is exhausted for economic reasons, arguing instead that it fails for political reasons and that its success "rides more on the strength of state autonomy and less on its inherent economic weaknesses" (p. 23). The second chapter of the book is devoted to the explication of the argument that rent-seeking behavior (which a strong, autonomous state would have been able to resist but weaker states have been unable to control) has led to the exhaustion of the ISI strategy. The analysis of the emergence of rent-seeking behavior under ISI and its political consequences is excellent. Attributing the eventual failure of ISI to the absence of an idealized version of the autonomous state impervious to sectoral, group, or class interests, however, is less than convincing. It may also have no empirical referent. I suspect that referring to state autonomy tacitly assumes a state that is relatively homogeneous, very tightly integrated, and highly coordinated in its actions, which, as recent experience has shown us, has not existed even in totalitarian societies.

The more interesting empirical question is *who* influences the activities of the state—and how and why—under the conditions of ISI? Chapters 3 and 4 review the evolution and the implementation of ISI in Turkey, and the following chapters provide some answers to these questions. Chapter 5 is a concise and complete review of

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the economic policies that have been followed by Turkey during and after the 1960s, that is, during the persistent application of ISI policies. The statistical information presented provides an invaluable reference for those who may want to compare the Turkish case with that of other societies.

Next, Barkey raises the question why economic policies like the promotion of exports were not adopted to alleviate such major problems as the extreme shortage of foreign currencies? The answer (which is political, he argues) lies in the "unwillingness to change and in the state's inability to act decisively to counteract domestic distortions" (p. 104). Chapters 6 and 7 examine the cleavages within the private sector and state–business relations in Turkey. The aim here is to identify the main features of a background that produces indecision in the business community and the state's inability to act decisively against "domestic distortions." Both chapters are well researched, detailed, perceptive, and well written. To my knowledge, they constitute some of the best available analysis of the politically relevant cleavages in the Turkish private sector and the multifaceted and complicated relations between the Turkish business world and the Turkish government. As such, they are mandatory reading for those interested in Turkey and highly recommended for students of the political economy of other countries.

Having come full circle, the conclusion reiterates the earlier idea that politics of indecision (deriving from the inability of a divided business community to produce policy choices and the inability of a weak state to liberate itself from the pressures of the business community in order to formulate appropriate policies to alleviate the economic and political problems that ISI generated) paved the way to the emergence of a bureaucratic–authoritarian regime. Whether this is entirely accurate requires further debate in light of the fact that the economic reform policies were initially devised under an elected government; that the military did not take over until after eight months; and that within a period of two years, the military tried to set the country on a path of restoration of the old policies, which was reversed only after an elected government took office.

More problematical is the fact that expressions like *autonomous state* and *autonomous state apparatus* are not sufficiently specified. Sometimes, what is meant appears to be a government that is above party politics. At other times, a bureaucracy that is free from political guidance comes to mind. Similarly, how an autonomous state would have reacted differently to the problems generated by the strategy of ISI and how it might have behaved differently in terms of economic policy are not spelled out. These reduce the effectiveness of the theoretical argument considerably.

Barkey's book may be evaluated at two different levels. At the normative level, he seems to favor ISI and an autonomous state that he thinks would retain it. Here, his argument is not sufficiently cogent and needs to be thought through in greater detail. At the analytical level, on the other hand, what we have is an excellent analysis of the consequences of import substitution on the economy, social structure, and political processes, buttressed by a description of the process by which an export orientation and integration to the international economy have come to prevail recently in Turkey. At this analytical level, Barkey's work merits not only praise but the attention of anyone who is interested in the study of the political economy of economic change and economic stabilization programs.

Istanbul University ILTER TURAN
University of Iowa

The Moral Collapse of Communism: Bland As a Cautionary Tale. By John Clark and Aaron Wildavsky. San Francisco: Institute for Contemporary Studies Press, 1990. 431p. $24.95.

The rapid and unexpected collapse of communism in Eastern Europe has raised some fundamental questions. Why did these systems collapse? Are we dealing with mistakes by individuals, or was it a shortcoming of such systems in general? Furthermore, what are the implications of this collapse for communist systems in general?

Focusing on the Polish experience, Clark and Wildavsky argue that the main reason for the disintegration of these political systems in Eastern Europe is to be found in the nature of their communist political economies. Given the close relationship between politics and economics in communist systems, such a collapse was inevitable. The failure of such systems to tolerate political pluralism and their reliance on central planning, rather than market prices, doomed them to failure. To correct this situation, the authors argue that the neo-Marxist theory of the state under capitalism needs to be turned on its head; for in reality, this theory provides "far better predictions and explanations for communist political economies than for the capitalist systems at which their criticisms are aimed."

Turning to the states of Eastern Europe, the authors argue that these states were ruled by a single class, which exercised control of the economy. Indeed, it is the fusion of power in countries such as Poland that destroyed the competition so necessary to highlight errors and the diversity necessary to correct them. As a consequence, efforts to reform such systems by administrative measures only made the situation worse.

This is a controversial but invaluable book. For too many years, neo-Marxists and their critics have tended to talk by, rather than to, each other. This book, however, provides a clearly articulated set of criticisms of the communist experience and could serve as the basis for a dialogue between neo-Marxists and their critics. In what ways is the Polish or East European experience different from that of the Third World, for example? Is the Clark and Wildavsky critique relevant only for a Leninist regime? Or is it possible to design a Marxist, non-Leninist pluralistic system that would avoid the shortcomings they outline? These issues are too important to be ignored.

The book could also serve as a useful tool in a graduate seminar on (former) communist systems by serving as a bridge between the old and the new. Not only does it highlight some of the deep-seated problems the new political systems in Eastern Europe are struggling with, but it also focuses attention on the key question of just what caused these systems to fail. Even if not all readers agree with the conclusions suggested by Clark and Wildavsky, they will certainly find the

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well-documented, carefully articulated arguments difficult to dismiss out of hand.

Woodrow Wilson Center for International Scholars DALE R. HERSPRING

Oil and Politics in the Gulf: Rulers and Merchants in Kuwait and Qatar. By Jill Crystal. New York: Cambridge University Press, 1990. 209p. $37.50.

Crystal's book was written before the August 1990 invasion of Kuwait, but it is a solid piece of research whose merit exceeds ephemeral journalistic considerations. The book should appeal to several kinds of audiences: regionalists interested in the Middle East and Persian Gulf states; international relations specialists who want to know more about this strategic area and would like some solid local history upon which to anchor their postwar analyses; and theorists of state development. These last should pay particular attention to this work, for its title is not designed to catch their attention. Although the book does not pretend to make a deep theoretical contribution, there is much primary source material (based on archival research) of great interest.

The study is divided into two sections, one on Kuwait and the other on Qatar. The book establishes the extent to which Kuwait is a product of the international balance of power that has protected it from both Iraqi and Saudi threats. The development of the society and economy, especially the labor market, are also examined and related to the occasional regional disturbances that have threatened the monarchy. The need for an outside, world-class power to maintain the sovereignty of the Kuwaiti state is a repeated theme in the country's history. The headlines of the postwar period (e.g., the expulsion of Palestinians) are the tried-and-true recipes by which the regime has dealt with potential disloyalty and rebellion on many occasions in the past. Recent events are remarkable for their scale, but no one of them is new to Kuwait's politics.

Crystal's book shows the strong analytical influence of Hanna Batatu's *Old Social Classes and the Revolutionary Movements of Iraq* (1978), though it does not approach Batatu's work in scope. Like Batatu, Crystal gives a clear account of the influence of tribe and clan in two modern states and how these have interacted with the development of a class sytem that grew up with the penetration of the world oil industry. Unlike Batatu, Crystal does not focus on the development of pan-Arabist ideology or political parties, largely because their role has been more subordinated to the politics of tribe and clan in Qatar and Kuwait than in Iraq. Jealousy, fear, rivalry, greed, and shifting alliances are present in all political systems; but tribal rivalry and the lack of state institutionalization in Kuwait and Qatar give these features a particular flavor that Crystal illustrates quite well.

Crystal argues that economic development in both Kuwait and Qatar shifted a traditional balance of power between the primarily political power of the sheikhs and the primarily economic power of the merchants. Imperial backing from abroad and growing oil revenues helped ruling families in each state to consolidate their control of the state apparatus. Largesse with oil revenue broadened rulers' base of support against contentious family members and rebellious merchants. The resulting "distributive state" collected the bulk of its revenues from international trade and then distributed them to the citizenry, foregoing the need to develop an extensive domestic tax base. The merchants were either paid off directly or given other benefits, such as strongly protectionist policies, to secure their loyalty. In Kuwait, the merchant class was stronger and better organized than in Qatar: the push for representation to check the monarch has been stronger there. The more extensive ruling family in Qatar was more prone to squabbling, which was often exacerbated by Saudi meddling. Qatar has therefore been less stable internally than Kuwait.

Theorists of state development will be most intrigued by the book's analysis of class conflict. The political economy of state development in both countries was determined by such factors as the pearling industry, access to long-range trade routes, the effect of nomadism on labor supplies, debtor–creditor relationships between workers and merchants and, at the bottom of the hierarchy, the existence of a slave class. The interaction of slaves, free workers, merchants, sheikhs, and international oil companies is provocative reading, even if the theoretical implications are unexplored.

This book should properly be titled *Oil and Local Politics in the Gulf*. The international politics of oil are left virtually untouched. This is unfortunate, because there are some issues that oil histories have not explained. Examples are Kuwait's exclusion from, and Qatar's inclusion in, the famous 1928 Red Line agreement limiting oil production in the entire Saudi peninsula and Mesopotamia and Gulf Oil's decision to drop out of the Iraq Petroleum Company consortium and participate in Kuwait. But Gulf Oil is not even mentioned in the book; and the politics of the U.S. company's working in tandem with the British company is totally unexplored, as are the nationalizations of the foreign concessions in the 1970s. Crystal's book excels primarily because of its treatment of domestic politics.

State University of New York, Albany GREGORY P. NOWELL

The Hidden Nations: The People Challenge the Soviet Union. By Nadia Diuk and Adrian Karatnycky. New York: William Morrow, 1990. 284p. $22.95.

In Search of Civil Society: Independent Peace Movements in the Soviet Bloc. Edited by Vladimir Tismaneanu. New York: Routledge, 1990. 193p. $35.00.

Given the blistering pace of political change in the present-day Soviet Union and in Eastern Europe, it makes sense for Western publishers to bring out books devoted to that region quickly. *The Hidden Nations* represents such a "fast book." Completed in mid-1990, it appeared before the end of the year.

The aim of Nadia Diuk and Adrian Karatnycky's study is to recount "the great awakening of peoples that is occurring throughout the Soviet Union", (p. 32). Toward that end, they focus upon what they call the "hidden nations," that is, the non-Russian peoples of the Soviet Union. It should be noted that the authors have written a "popular" account, geared at a broad readership. In addition to serving as an essay in contemporary history, the book also advances public policy recommendations. This is to be expected from two experienced Washington hands: Diuk is Soviet and East European specialist at the National Endowment for

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Democracy, while Karatnycky is research director of the AFL–CIO Department of International Affairs.

As an essay in contemporary history, *The Hidden Nations* is a solid contribution to the mushrooming literature on what used to be called "the nationalities question." In addition to scrutinizing the Soviet press, the authors conducted three research trips to the Soviet Union during 1989 and 1990 in the course of which they visited a number of the Soviet republics. The authors generally come across as well informed, and I found few factual errors in their wide-ranging account.

Diuk and Karatnycky's public policy recommendations could be summed up as an appeal for common sense on the part of Western governments. Power, they maintain is rapidly devolving to the republics. It is thus the republics, not the Soviet "center," that should be the chief object of Western attention and largesse. They also note, appropriately, that since the mid-1970s there has been occurring a significant out-migration of Russians from the Soviet periphery and that this migration pattern objectively serves to facilitate the devolution of power to the republics.

The authors contend that the West should offer "consistent moral and material support for those indigenous movements in the USSR that favor democracy and reject chauvinism" (p. 248). The West, they believe, should attempt to encourage and to strengthen the civil societies that are emerging in the republics (albeit at different speeds) and that could soon result in a number of the republics' becoming authentic Western-style democracies.

While *The Hidden Nations* exhibits numerous strengths, it also suffers from two important failings. First, there are no notes. A brief Sources section at the end of the volume fails to compensate for this glaring lapse. In the absence of notes, there is obviously no way a reader can check the accuracy of the many direct quotations that appear.

The volume's second failing is a conceptual one. The authors consistently advance the thesis that the Soviet Union must be seen as a "Russian empire" and that ethnic Russians objectively play the role of an "imperial nation nonpareil" (p. 37). For the authors, the continuities conjoining prerevolutionary Russia and the Soviet Union far outweigh any discontinuities. While Diuk and Karatnycky frequently employ the term *totalitarianism*, it seems that, for them, totalitarianism serves as a "mask" for the reality of ethnic Russian rule. "For the non-Russians," the authors assert categorically, "Marxism–Leninism is an alien ideology", (p. 39). This implies that for ethnic Russians Marxism–Leninism constitutes a form of national ideology.

This thesis cannot withstand careful analysis. The late nationalities specialist, Hugh Seton-Watson, was wont to argue that the historic tsarist empire should not be seen as an ethnic Russian empire. The tsarist empire, he noted, was based on the principle of autocracy, not any principle of ethnic Russian colonial superiority. All peoples living in the empire therefore enjoyed the emperor's protection regardless whether they were Russian by speech or religion. It was only toward the end of the nineteenth century, Seton-Watson argued, during the reigns of Alexander III and Nicholas II, that the tsarist government began to adopt a conscious policy of Russification, a course justified not in the name of the monarch but in that of the Russian nation.

If the tsarist empire cannot, except in its closing years, be termed an ethnic Russian empire, much less can the Soviet Union be so considered. As Carl Linden has pointed out: "The [Soviet] party–state in its strict conception acts not in the name of those it rules but as a proxy of the future 'communized' humanity freed of political rule and no longer separated into nation–states. By its ideology the Soviet party–state is the revolutionary replacement of the Russian nation–state, not its continuation", (*The Soviet Party–state* [1983], 95). Marxism–Leninism hardly serves as a "veil" for ethnic Russian imperial ambition.

Since the Soviet Union cannot be considered a Russian Empire, it is clear that ethnic Russians cannot be termed an "imperial people." As Alain Besançon noted in 1983, the position of Russians in the Soviet Union is dissimilar to that of, say, the English or the French in the British and French empires. Besançon maintained:

> To have an empire one must have a privileged people, an essentially military means of conquest, and limited goals. The Russian people has no privileges. It has "advantages" certainly, as the surest ally of Communism. . . . [But] these advantages are not rights. Moreover, they are compensated for by heavier obligations, exemption from which is considered by the Russian to be a privilege of the non-Russian nationalities. A Russian who enjoys privileges as a Communist owes his privileges to his Communism, not to his Russianness. (In ed. Robert Conquest *The Last Empire*, [1986] 10–11)

Unfortunately, Diuk and Karatnycky fail to see what Seton-Watson, Linden, and Besançon see so clearly. With the exception of this conceptual lapse, the authors have written a useful book that can help us better to understand the dramatic "awakening of peoples" presently taking place in the Soviet Union.

In contrast to *The Hidden Nations*, *In Search of Civil Society* is an example of a "slow book." A number of the constituent chapters were completed by early 1988, but the volume itself did not appear until late 1990. Despite the dated nature of some of the material, however, the book retains considerable interest for specialists.

The goal of the collection (whose editor and principal author is Vladimir Tismaneanu of the Foreign Policy Research Institute) is to focus upon the "grass-roots, unofficial, nongovernmental peace movements" (p. viii) that emerged in the Soviet Union, Poland, Hungary, Czechoslovakia, and East Germany during the course of the late 1970s and the 1980s, and to treat these movements as case studies in the emergence of "civil society" in Eastern Europe. This exercise is a worthwhile one, since it is clear that the "year of miracles" (1989) in Eastern Europe and the formation of "sovereign" Soviet republics did not occur ex nihilo. Tismaneanu and his coauthors are correct in affirming that the "islands of autonomy" that were staked out, often with great difficulty, in Eastern Europe during those decades bear a direct causal relationship to the collapse of communist regimes at the end of the 1980s.

In his illuminating contribution to the volume, Hungarian social critic and human rights activist Miklos Haraszti theorizes that Eastern European societies must pass through three distinct "eras" before they are able to cast off the communist yoke. The first stage he posits is *post-Stalinism*, during which the party–state reduces repression to a "necessary" level and permits a limited degree of consumption and of economic decentralization. During this stage, one observes the emergence of independent initiatives not directed by the party–state.

The second stage in Haraszti's scheme is *posttotalitarianism*, during which an authoritarian regime is increasingly placed on the defensive by a burgeoning civil

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society. The party–state's legitimizing ideology, Marxism–Leninism, suffers a complete collapse; but the party structure remains in place.

The final stage—which occurred in Eastern Europe after Haraszti had predicted it in early 1988—is *postcommunism*, under which political democracy is installed. "What is significant," Haraszti observes, "is that without the evolution of civil society in the preceding two phases, the transition cannot be successful" (p. 86). Haraszti's scheme could, one suspects, also be of help in predicting future developments in non–East European communist societies.

The emergence of independent peace movements in the Soviet Union and Eastern Europe was preceded by "islands of autonomy" created by rock musicians and their fans (the "Lennonists," after John Lennon), environmentalists, and those seeking the preservation of historical and cultural monuments. The formation of independent peace movements was, however, especially significant, since these entities served to challenge the ruling communist regimes on closely guarded turf; the campaign for "peace" (i.e., the inducement of Western unilateral disarmament) had been a key element in the foreign policies of those regimes. The at-times-brutal contest between the independent peace movements and the regimes makes a fascinating story, as do the fractious ties that developed between the movements and various peace organizations in the West.

To sum up, *In Search of Civil Society* is a collection of stimulating essays serving to broaden our comprehension of the processes that led to the sudden collapse of communism in Eastern Europe and that could presage its imminent breakdown in the Soviet Union.

Hoover Institution JOHN B. DUNLOP

Politics, Economic Development, and Industrial Management in India. By Howard L. Erdman. Hanover: University Press of New England, 1989. 246p. $35.00.

Howard Erdman's book is a well-developed case study of a typical industry in India from its conceptualization stage to the present. Erdman justifies the selection of Mysore Chemicals and Fertilizers as a valid case on the ground that its checkered performance is representative of the overall industrial performance in the nation. The objective of this painstaking exercise (as the author himself claims) is to reach those who are either interested in the field of policy analysis or keen to move away from macro studies to the micro case studies that offer a narrow—albeit focused—view of political phenomena. The political phenomenon that Erdman explores is the effect of the political environment on the process of industrial decisionmaking.

The case study of Mysore Chemical and Fertilizers is comprehensive and provocative. Several dimensions of the "political environment" are carefully explored by the author. Under the institutional dimension, Erdman analyzes India's complex federal system and the interrelationship between the center and the state of Karnataka. The policies of the central government can—and do—cripple industrial initiatives. The author also makes a genuine effort to highlight the drawbacks of the Indian style of "mixed economy" by bringing in issues of the joint and semipublic sectors. Erdman interestingly points out that although the government's power in barring people from doing what it deems undesirable is clear, its power is limited when it comes to making people do what it deems desirable.

Under the social and psychological dimensions, the case allows the author to venture into two fascinating areas: caste politics and personal politics. Erdman concludes that the domestic political environment is important but that at the same time the "individual matters." While Erdman raises these issues in several places, he fails to explore them systematically. Caste identities play an important role in the selection of the chairman. But do they also play a role in the management of a company? To what extent are these positions used as tools to muster support for the ruling party? Is the politicization of management a cause or a consequence of the company's failed performance?

The historical account of Mysore Chemicals and Fertilizers from 1960 to 1987 is a powerful case study in itself. However, considering its richness, the framework in which the case is placed is disappointing. Rather than developing a theoretical framework by reviewing one primary body of literature, Erdman plays "touch and go" with the names of hosts of scholars. The resulting format is less like a scholarly work and more like a graduate seminar. This format is also prevalent in the concluding section, where the only conclusions drawn are that every model and every phenomenon identified in the introductory chapter is present in this case. Little attempt has been made to assess their comparative significance, to link the case with other similar cases in India, or to relate them to other studies in the field. Although the author claims that the book is useful for comparative purposes, the burden of finding its comparative worth is left on the reader.

While the case study is undoubtedly interesting, it is not an analytically rigorous work. The absence of any specifically stated propositions, operationalization of concepts, or presentation of proofs mars the study's value for wider audience. Much is left to the reader's speculation. The lack of any hard data on a subject where data abounds is also disturbing.

The readability of the book is greatly hampered by the author's preference to abbreviate almost everything, including the names of people. Often, four or five abbreviated names appear in one sentence.

Overall, the chief strength of the book is the case study itself, which is not only interesting but insightful as well. Even though it does not break new ground, it is meaningful to those interested in Indian politics.

University of South Florida RENU KHATOR

The Three Worlds of Welfare Capitalism. By Gosta Esping-Andersen. Princeton: Princeton University Press, 1990. 248p. $37.50 cloth, $12.95 paper.

Modern Welfare States: Politics and Policies in Social Democratic Scandinavia. By Eric S. Einhorn and John Logue. New York: Praeger, 1989. 340p. $55.00 cloth, $17.95 paper.

Age, Class, Politics, and the Welfare State. By Fred C. Pampel and John B. Williamson. Cambridge: Cambridge University Press, 1989. 199p. $34.50.

As their titles suggest, the three books reviewed here concern the social welfare state. Of the three, *The Three Worlds of Welfare Capitalism* constitutes the most vigorous

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attempt to move beyond conceptual categories that now dominate the field. The author, Gosta Esping-Andersen, replaces notions of the welfare state that emphasize total amounts of expenditures on social programs or the degree of redistribution that the system realizes with a classification of programs according to whether the programs are universalistic and enable persons to maintain livelihoods without reliance on the labor market. The purpose of social rights, Esping-Andersen says, is to "permit people to make their living standards independent of pure market forces" (p. 3). She argues that with respect to living standards, social rights are supposed to enable citizens to be more than mere commodities. They should lead to the "decommodification" of citizens.

With this idea of the purpose of social rights in mind, she draws distinctions between three types of welfare state. First, the *liberal* welfare state mixes nonuniversal means-tested programs for the poor with programs for all who contribute to them based on a social insurance foundation. Second, the *corporatist–statist* welfare state emphasizes social insurance programs that benefit those who pay in—a larger number than in the liberal state but still excluding some sections of the population (e.g., homemakers). Third, the *social democratic* welfare state emphasizes programs that are universalistic (i.e., independent of whether people do or do not pay in) and also grant benefits that are tied to a middle-class style of living. This third type of welfare state thus enables an able-bodied person of working age who so wishes to live independently of the labor market for long stretches of time (e.g., when unemployed, sick, or disabled or to pursue education or raise children).

Esping-Andersen's book is intriguing in its suggestion of fresh hypotheses about the welfare state. First, she uncovers little correlation between the gross national product of a nation and the type of welfare state a nation has; her comparison of 18 Western countries shows that the politics of a nation is a far more potent explanatory variable. Second, tax revolts against the welfare state tend to be strongest not in nations for which the welfare state poses the greatest tax burden (the universalistic systems) but in those that spend the least on welfare. Also important is the connection she draws between the commitment of a system to universalism and the dependency of such systems upon full employment in order to garner the necessary financial resources to fund the system. The result is that though they may seem on the surface to be indifferent whether people work, universal welfare systems are instead geared toward attempting to increase employment with the consequence that these systems are associated with higher levels of employment than the other two types of welfare systems and often achieve full employment. These comprise a few of the ways in which Esping-Andersen's approach invites rethinking a wide range of conclusions that presently have wide currency in the field.

Yet drawbacks also exist. The author mentions at the start that her need to compress a lot of information into a small space for the purposes of developing measures for comparison might lead some to feel that her descriptions of the various welfare states are superficial and perhaps misrepresentative. I had this feeling on some occasions with respect, for example, to the West German and U.S. welfare systems. Also, her emphasis on independence from the labor market as a key element of the definition of the universal welfare system needs to be spelled out in greater detail. After all, the universal welfare system itself places limits in the amount of time people will be supported without working. What defends these limits placed on support if independence from the labor market is a vital criterion? Also, the financial viability of a universal welfare system may well depend upon a work ethic operating in these countries that infuses people with the belief that it is *not* acceptable to be independent of the labor market except in special situations. Quite a few anomalies exist, unfortunately, that Esping-Andersen, in this otherwise excellent work, leaves not only unanswered but mostly unaddressed.

A companion to Esping-Andersen's book is Eric S. Einhorn and John Logue's *Modern Welfare States*. The book describes in detail the welfare systems of the universalistic type as they operate today and as developed historically in the Scandinavian nations. Although the book's explanations are impressionistic in nature rather than backed by systemic evidence, they are nonetheless imaginative and worth consideration. According to Einhorn and Logue, the universalistic welfare state arose in Scandinavia due to causes reaching well beyond economic development—which is generally in accord with Esping-Andersen's conclusions. A key factor behind its emergence, in Einhorn and Logue's view, was the popular belief system—in particular, the belief in solidarity, that is, a sense of belonging to, and identifying with, others and a willingness to tie one's fate to that of others. This variable, imbedded in the political culture of the nations, loomed of greater importance than economic variables.

Einhorn and Logue's analysis reaches even further. It also focuses on the decline of the consensus favoring the extensive welfare state. The decline of consensus developed in Scandinavia following the mid-1960s. Here economic variables do move more center-stage. The authors suggest that the affluence of the age "undermined the appeal of the social welfare system. Economic insecurity seemed increasingly remote" (p. 20). Political variables also played a role, in that in the late 1960s and early 1970s there emerged value issues (environmental conditions, nuclear power, entry into the European Community that cut across traditional class lines in a manner as to reduce the cohesion of the vote for the Left. Most importantly, however, Einhorn and Logue observe that a universalist welfare system (as Marx said of a capitalistic system) may contain the seeds of its own demise. A universalistic system builds upon a feeling of solidarity that gives rise to the system and is needed to sustain it, the authors argue. Yet by its very nature, the operation of a universalistic welfare system works to undercut solidarity values. The very purpose of the system is to enable individual citizens to be economically independent of one another, whereas in prior times ties to families and friends were required to attain material security. "The welfare state," say Einhorn and Logue, "diminishes the reciprocal responsibility of each individual for his or her family, neighbors, and workmates. . . . It is ironic that the collectivist means that conservatives always feared would destroy individualism have, in fact, encouraged it" (pp. 267–268). An important and intriguing hypothesis, it unfortunately remains little more than that, since beyond their logic, the evidence the authors summon comprises little more than a quote from a Swedish actor.

The third book, Fred C. Pampel and John B. Williamson's *Age, Class, and the Welfare State*, is also of consid-

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erable interest, although its denser prose makes it more difficult to read than the other two books. If the book by Esping-Andersen reveals the impact that conceptual understandings can have on conclusions about the welfare state, Pampel and Williamson do the same with methodological issues. Their study of the welfare state differs from the conventional comparative methodology in that it focuses on longitudinal rather than cross-sectional data for the nations under study and simultaneously examines the several constituent parts of the welfare state separately over time, rather than only the whole or a single program within the whole. From this different methodology emerges a series of conclusions that conflict with those derived from the conventional methodologies. First, political variables once more come to the fore in explaining change in expenditures. Although economic growth remains a critical variable for change in expenditures on the largest programs (social insurance and health care), the political context is found to be an important mediating variable. Particularly powerful are changes in the percentage of the population that votes and the degree of electoral competition that exists. Moreover, for the smaller programs (e.g., public assistance, family allowances, and unemployment benefits), Pampel and Williamson find that economic growth is unimportant. More influential in the development of these programs are changes in the scope of union power in the nation and also, in the case of family allowances, the percentage of the population that votes.

University of Arizona JOHN E. SCHWARZ

States and Urban-based Revolutions: Iran and Nicaragua. By Farideh Farhi. Champagne: University of Illinois Press, 1990. 147p. $29.95.

Of all the conceptual challenges facing the comparativist, the study of revolutions is perhaps the most difficult. In the quest for comparability and equivalence, the student of comparative revolutions needs to consider the total millieu of the revolutionary situation—politics, culture, economics, sociology, and social psychology. All too often, this has been an impossible task. To complicate matters, most scholars tend to focus on successful revolutions, rather than failures. Social scientists might do well to accept Thomas H. Greene's painful but audacious assertion that it may be easier to carry out a revolution than to understand it.

The author of the book at hand is well aware of the immensity of these conceptual and analytic difficulties. Yet Farideh Farhi is courageous enough to attempt a comparative macroanalysis of two urban-based revolutions—the overthrow of the Iranian monarchy and the Samoza regime in Nicaragua.

Using a case study approach, Farhi takes Theda Skocpol's theory of revolution as a starting point and proceeds to develop her own explicit framework for application to the Iranian and Nicaraguan situations. She presents a cogent critique of Skocpol's approach, which she finds wanting in understanding societies that lack agrarian bureaucracies and peasant insurrections. In Farhi's view, Skocpol's theory can become more pertinent to recent revolutions if focused on the changing balance of class forces occasioned by the uneven development of world capitalism and by a broader view of ideology stressing the importance of ideas, actions, and interests. Farhi's specific concern is to understand the interaction of class and state structures within the global political and economic system. Her findings can be summarized under three headings.

First, the impact of sociostructural and world historical forces on the economic sytems of Iran and Nicaragua produced a coalescence of urban groupings able to play an oppositional role against two economically interventionist regimes that favored development policies based on the export of primary products. The disoriented urbanization process created a readily mobilizable population.

Second, the coalescence of the urban groupings was actualized because of the disjunction between the ruling elite and the intermediate classes. Political parties were prohibited and the rising professional classes were antagonized. The coalescence of opposition groups was also related to the effective use of the existing religiocultural networks and ideologies based on religion, which offered the revolutionaries a medium of nationalist expression.

Third, the breakdown of the state machinery and its inability to respond to popular mobilization was a consequence of the peculiar structure of the two regimes, a world milieu of superpower rivalry, and confusion among U.S. policymakers.

In a concluding section, Farhi identifies certain similarities between the state-centered crises that launched the Iranian and Nicaraguan revolutions and those that triggered the upheavals in Mexico (1910–20), Cuba (1956–59), and the Philipines (1986). In the end, she returns to pay homage to Skocpol, her original source of inspiration, while underlining the difficulties inherent in applying her hypotheses to Iran and Nicaragua. One finds Farhi's sense of loyalty somewhat exaggerated, in view of her major departures from Skocpol that render the latter's theories almost unrecognizable. Farhi's focus on urban mobilization, ideology, urban oppositional groupings, and the vulnerability of peripheral states to internal and global pressures make her work significant in its own right.

Yet Farhi's commitment to her particular approach tends to deemphasize or hide such salient factors as the role of revolutionary leadership and the explosive interactions of culture and class. There is little in this study about the epicentric role of Ayatollah Khomeini and the Sandinista leadership in contributing to the success and shape of the revolutionary outcomes. In the Iranian case it would have been helpful to note the peculiar convergence of class and cultural cleavages that produced a revolutionary environment in which the urban *lumpenproletariat* was readily mobilized, since it suffered relative deprivation both in cultural and economic terms. On the crucial revolutionary role of the intermediate classes, the author might have consulted Leonard Binder's seminal work on Egypt (*In a Moment of Enthusiasm: Political Power and the Second Stratum in Egypt*. Chicago: University of Chicago Press) that goes beyond Gramsci to Marx and Mosca to demonstrate how a section of civil society can arouse in itself and in the masses a moment of enthusiasm and acts as the general representative of the whole society to achieve a partial revolution.

Nonetheless, Farhi's work is a fine example of how a doctoral dissertation can be transformed into a good book—a welcome addition to the literature on comparative revolutions.

University of Southern California R. HRAIR DEKMEJIAN

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Coffee, Contention, and Change in the Making of Modern Brazil. By Mauricio A. Font. Cambridge, MA: Basil Blackwell, 1990. 351p. $49.95.

Comparative historical sociologists, including Barrington Moore, Theda Skocpol, Charles Tilly, and Fernando Henrique Cardoso, have contributed much to the study of economic, political, and social change. Mauricio Font's analysis of the dynamics of change in the state of São Paulo leading to the Brazilian Revolution of 1930 fits into this analytical tradition.

Font emphasizes the impact of changes in Brazil's agrarian export sector that contributed to the "onset of industrialization and the demise of the Old Republic in Sao Paulo" (p. 270). Differing with some of Brazil's most respected sociologists, including Florestan Fernandes, he challenges the often-advanced argument that "development and industrialization in Sao Paulo emanated from processes of capitalist accumulation linked to large planters" (pp. x–xi). Font contends that the emergence of independent agriculture and the associated competitive economy, arising outside the large *fazenda*, contributed significantly to this process of change. His assertions are sufficiently well documented in primary and secondary historical materials to merit close examination.

Font emphasizes two dimensions of São Paulo's independent agrarian economy in the 1920s, 1930s, and 1940s: (1) coffee production on small farms of less than 50 hectares, or 20 thousand trees and (2) the cash crop production—predominantly on small farms—of maize, beans, rice, cotton, and sugarcane (pp. 14–18). Like *planters* in the *fazenda* system, small landholders in the independent coffee sector produced for export, while the independent producers of other crops targeted internal markets. The two activities—often combined on the same farm—arguably produced a class of farmers and a competitive economy that operated relatively independently of the large coffee producers and their commercial agents. Their success gradually eroded the social and political influence of the planters. Font concludes, "By the early 1920s an alternative coalition made up of a growing mass of small and medium holders, land developers, new commercial intermediaries, and professional politicians was challenging the economic pre-eminence of established large planters" (p. 29).

These developments, Font asserts, contributed much to the demise of the old republic and the emergence of industrialization. The shortcomings of the *fazenda* coffee economy and the emergence of an "alternative agrarian economy rooted in a mode of production different from the very large established estates" created "expanded opportunities for capitalist industrialization and development" in São Paulo (p. 107).

For political scientists, Font's documentation of the process in São Paulo by which the "state" gained autonomy from the "established elites" is especially important, both factually and theoretically. He demonstrates that the political influence of the planters declined as a consequence of the emergence of an "alternative statist coalition" composed of "independent agriculturalists" and "fledgling industrialists" who benefited from São Paulo's growing export economy (p. 271). At least in the Brazilian case, state autonomy increased when society became more pluralistic.

Font enhances the value of his book by demonstrating how his findings challenge or support those of other scholars. For instance, he refutes André Gundar Frank's thesis that "specialization in the production of primary products dooms countries and regions to subordinate positions in an international division of labor and thereby to underdevelopment and a rigid social structure" (p. 272). Instead, Font shows how "export [agrarian] sector expansion can lead to substantial forms of social change and development" (p. 273). Also, Font illustrates how the development of an "alternative economy" in São Paulo that "fueled the domestic market, provided cheap inputs to other sectors of the economy, and made possible the continuous experimentation with small-scale accumulation and entrepreneurship" can be characterized in terms of Albert Hirshman's notions of "backward" and "forward" linkages. Additionally, he contrasts the *fazenda* system and the "alternative economy" in terms of Alain de Janvry's notions of "disarticulation" and "articulation." These conscious attempts to compare and contrast his analysis with that of other scholars enhance the theoretical value of the book.

Surely, this book will evoke criticism from scholars whose work it consciously challenges. It cannot be ignored. The debate will not be definitively resolved, however, since both Font and those whose findings he has questioned can find "facts" in the historical record to confirm their respective claims. The critical dialogue enjoined by Font, however, cannot help but clarify our understanding of the dynamics of economic, social, and political change.

Font does not address how changes in the global political economy affected Brazil's 1930 revolution. Erosion of the *fazenda* system, at least in part, was the product of the decline in coffee prices. In the late 1920s and early 1930s, the world supply of coffee steadily increased as producers from other countries entered the market; at the same time, coffee consumption slowed. These reversals, coupled with the Great Depression, undoubtedly contributed to Brazilian revolution. Font's neglect of these and other international factors weakens his analysis.

Still, Font's book contributes to our understanding of Brazil and to the theoretical literature on the dynamics of economic, political, and social change.

Brigham Young University W. Ladd Hollist

Fire in the Rain: The Democratic Consequences of Chernobyl. By Peter Gould. Baltimore: Johns Hopkins University Press, 1990. 163p. $19.95.

The Truth about Chernobyl. By Grigori Medvedev. New York: Basic Books, 1991. 274p. $22.95.

Five years after the nuclear and environmental disaster at Chernobyl dramatically captured the world's attention and in light of new information showing that the magnitude of this catastrophe was, and continues to be, much worse than had been thought, two excellently written and genuinely gripping accounts are available that attempt to tell the story behind the worst nuclear accident in history. In many respects, Chernobyl will forever represent a truly reflective type of Promethean apologue whose moral and ethical messages extend beyond the geographic, politico-ideological, or cultural boundaries of any one nation—certainly beyond the chain of cataclysmic events that so memorably took place on 26 April 1986.

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Peter Gould's *Fire in the Rain* not only tells the story behind the Chernobyl accident (which he has written in an informative, scientifically demystified, and most absorbing style) but also raises a number of keenly perceptive questions about the extent to which the dissemination, manipulation, and suppression of information about the accident and its effects may have been influenced by myriad politico-economic considerations, and, perhaps most significantly, by the degree to which a country is dependent upon atomic power. Gould, who, during the research for this book, covered well over five thousand pages of official reports, eyewitness statements, scientific treatises, and media accounts, convincingly tries to capture this important correlation between a nation's dependency upon atomic power and the degree to which information about the Chernobyl accident and its effects was manipulated or suppressed for no less than 26 West and East European countries (pp. 112–19). At minimum, the nuclear accident at Chernobyl created its own complex information crisis from which all nations can and must learn, and Gould's examination of the range of governmental reactions and responses—especially how and when information was made public in both Eastern and Western Europe—is impressive and instructive.

Grigori Medvedev's *Truth about Chernobyl* also tells the story leading up to the accident and its subsequent cover-up. Medvedev was chief engineer at Chernobyl when it was being constructed during the 1970s and at the time of the 1986 accident was deputy director of the Central Directorate for Power Station Construction in Moscow (a department of the Ministry of Energy), where he was in charge of construction at nuclear power stations. Within days of the accident, Medvedev returned to Chernobyl to investigate the magnitude of the disaster officially.

The Truth about Chernobyl is an engrossing and masterfully written account based upon interviews with the directors, engineers and others who worked at the plant, the firefighters and helicopter pilots who valiantly tried to extinguish the fire, eyewitnesses to the explosion, the doctors and nurses who treated radiation victims and risked their lives to save others, and many key people who died from radiation within weeks of the accident. Medvedev's book is, without question, the most authoritative statement on the Chernobyl accident to date. Indeed, in the preface Medvedev notes that he sought to answer a number of crucial questions he believes have been on the minds of many in both the Soviet Union and abroad: By what route did we arrive at Chernobyl? What actually happened, especially on the night of the explosion and over the next few days? What caused the disaster? What were its consequences? What were the lessons of Chernobyl? What does the future hold? (p. x).

Publication in the Soviet Union of *The Truth about Chernobyl* in 1989 was itself somewhat of an eventful test of glasnost, Medvedev's uncensored account quite legitimately taking its place among the growing number of precedents by which we assess the "new thinking" behind Gorbachev's media and information policies. Published in the United States to coincide with the fifth anniversary of the accident, *The Truth about Chernobyl* is skillfully translated by Evelyn Rossiter and contains a genuinely important forward by the late Andrei Sakharov.

What are the lessons and democratic consequences of Chernobyl? To what degree has the economic and political fallout from the accident increased public opposition to the future of nuclear development in the Soviet Union and abroad? Should nuclear power continue to be developed? If so, who should define and direct the course, tempo, and extent of such development? By asking such questions, both Gould and Medvedev raise a number of crucial socio-scientific issues that certainly predate Chernobyl and once again highlight fundamental concerns that continuously remind us "that humankind is still trying to come to grips with the fantastic, powerful forces which it has brought into being, and is still only learning to use them for the sake of progress" (Mikhail Gorbachev, speech 18 August 1986). In many respects, Gould articulates this delicate relationship among the physical, living, and human worlds as simply and effectively as anyone can: "The problems of atomic energy programs are not scientific problems in any strict sense. They are moral problems, and they arise from the apparent difficulty many have of moving from the demonstrating of the inevitable to the deliberation of the variable" (p. 145).

Among the most important lessons of Chernobyl is the question it raises about how governmental bureaucracies, politicians, and scientists (whatever their political or ideological suasion) manage an atomic or ecological crisis that knows no political or ideological boundary. Both works also raise a number of questions essential to the future of nuclear energy throughout the world: Who should be involved in making decisions regarding the development of atomic power, the location and construction of nuclear power stations, and the disposal of radioactive waste? In sum, who should define the issues and make the decisions that will, without doubt, have an impact upon the delicate balance among the physical, living, and human worlds? And whatever direction the development of nuclear power takes in the future, Andrei Sakharov (himself an advocate of nuclear energy) warns us, we must not leave the answers to these crucial issues solely to technical experts—still less to bureaucrats (p. viii).

In sum, both *Fire in the Rain* and *The Truth about Chernobyl* are excellently written works of gripping detail that are, without question, of essential benefit to anyone interested in the history of nuclear power development in the Soviet Union, the way nuclear crises and information about such crises are managed (or mismanaged), the impact the accident had upon the countries affected by Chernobyl's radioactive fallout, the lessons and democratic consequences of the world's worst nuclear disaster, and the future of atomic energy in both the Soviet Union and abroad.

Soviet Academy of Management JOHN MICHAEL CARFORA

The Economics of Political Violence: The Effect of Political Instability on Economic Growth. By Dipak K. Gupta. New York: Praeger, 1989. 329p. $49.95.

Gupta's book is a wide-ranging, high-quality work in the best traditions of the new political economy. The author is clearly well-trained in both political economy (including far more than public choice and economics) and statistics. The book contains an interesting and original model of political rebellion and a careful, methodologically sophisticated study of two aspects of the

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political economy of violence in 104 noncommunist countries: the effect of violence or rebellion on their economies and an attempted estimate of the basic behavioral model of individual behavior. Its gravest fault can be easily overcome by a willful reader: the literature review sections are too long. The informed reader will want to skim these chapters (primarily chaps. 2 and 3) quickly and get down to business. And the business is rewarding.

Gupta begins with a general theoretical model of political violence. In doing this he builds on the obvious (and by now generally accepted) insight that the great sociological deprivation theories of political rebellion (e.g., Merton's) did not tell us when individuals will *not* engage in violence. To do this, Gupta uses the best ideas from the rational choice school, along with other psychological and sociological factors. He notes that to get to reasonable levels of participation in a rebellion by rational individuals, one has to get beyond a classical, self-interested model of rationality. His moves in doing this may not be to the liking of all informed public choice types; but it is clearly in keeping with the earlier ideas of Brian Barry and others in their critiques of the Downsian voter.

To get beyond these earlier discussions, Gupta makes a smart move: he introduces the notion of *ideology* as a *preference for benefits* flowing to the groups to which the individual belongs (p. 142). (One might wonder whether the model would not be richer if it were defined in terms of groups with which the individual personally identifies and groups that the individual would like to deprive.) This permits the author to introduce such sociological variables as group consciousness, cohesiveness, and mobilization into his theory. Mobilization, Gupta notes, really requires an organizational component, as well—and some expectation of positive gain. The latter clearly relates to counterrevolutionary sanctions or coercion. All this is nicely integrated in the model put forward in Chapter 5.

Gupta then employs calculus to show the marginal conditions that must be satisfied for participation to be rational. These are used to develop models of likely cycles of violence in systems with specific parameters. Some of these are later estimated with the data. In the arguments that flow from the model, it would appear that the author had an eye toward asking questions regarding the current U.S. experience, namely, considerable continued deprivation for our minorities and yet relative absence of serious political violence. But these questions are not developed in the book as it moves to an aggregate political economy level to investigate the effect of violence on economic growth.

Data from 1948 to 1952 from the *New York Times* are presented for 104 countries, but a far more restricted subset of years is statistically manipulated to do the data analysis. The analysis is quite broad and includes simulations of income redistribution patterns to generate economic growth, the prediction of political violence in countries, the loss of income attributable to instability, and so on. The data analysis is extraordinarily interesting, even when some of the results are only marginally significant, and others not surprising. The program of statistically testing a micro behavior hypothesis with macro system data is problematic, but, at least, the author seems aware of the pitfalls. The limitations of the results are reflected in the relative thinness of the policy conclusions.

These disappointingly thin levels of significance can be looked at in at least one very positive light. Given the strength of development of the approach and the statistics developed, students of political economy, political violence, and political stability have been given food for thought that should serve them in good stead for the development of the next generation of studies on these subjects. And if these students' tastes do not lead them to begin with the material presented in this book, they are likely to be embarrassed by their critics.

University of Maryland, College Park JOE A. OPPENHEIMER

The Battle for Britain: Thatcher and the New Liberals. By Stephen Haseler. London: I. B. Tauris, 1989. 195p. $24.50.

Cutback Management in Public Bureaucracies: Popular Theories and Observed Outcomes in Whitehall. By Andrew Dunshire and Christopher Hood. New York: Cambridge University Press, 1989. 253p. $65.00.

Local Government and Thatcherism. By Hugh Butcher, Ian G. Law, Robert Leach, and Maurice Mullard. New York: Routledge, 1990. 171p. $48.00.

These three works on Thatcherism and policy will be valuable for scholars of either British policy or comparative conservative movements in the 1980s. Haseler has written a polemic essay from a neo-radical perspective, while Dunshire and Hood and Butcher and his colleagues both provide full sets of arguments and data on the effects of government policy changes. These two are complementary: both adopt official figures and match them to policy goals, but Dunshire and Hood take the Whitehall level while Butcher and his colleagues examine the effect at the local government level. It was, of course, conflicts with local government spending and taxation that partly undermined Mrs. Thatcher's final administration of 1987–90.

Haseler presents a clue to the ability of the radical Right in Britain to co-opt members of the center during the 1980s. He has shifted from the Labour party and Greater London Council to the Radical Society, Social Democratic Party, and Alliance. Yet the essay presents not opposition to, but startling agreement with, the Thatcher government.

The steam engine of this readable essay is the liberalism of the early nineteenth century radicals, Cobden and Bright. The Victorian values of Mrs. Thatcher and those of Haseler overlap in the social agenda and economic prescription, if not in partisan standard. Cobden and Bright stood for free trade and the abolition by mobility of the old, stultifying, feudal class relationships; they lamented that the industrial revolution had made millowners and merchants wealthy only to find them sycophantic in face of the feudal aristocracy.

The journalist in Haseler might appreciate William Safire's distinctions between the *libright*, or libertarian strain (Adam Smith) of liberty in social and economic markets and the *tradright*, or traditional strain (Edmund Burke) based on a natural order of governing through the upper social class. The passing of the conservative leadership to a younger "one of us" (self-made man) certainly fits the social change emphasis of the volume, which polemicizes *against* the paternalistic, "allegiant" society (as portrayed by Philip Norton) and *for* a more

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U.S.-style open social order. It is weaker in qualifying the argument in the light of real policy in the United States and the United Kingdom, and there is no explanation or critique of public choice theory.

There is little analysis of Mrs. Thatcher's partisan "handbagging" of other institutions, such as the Greater London Council (replaced with unradical appointive boards). Similarly, her rock-solid support for the police and intelligence services, her political (if not budgetary) support of the armed forces, and her vociferous opposition to the European Community's development, are all testimony to a traditional conservatism of sovereignty, security, and state power abroad.

One would like to see more systematic dissection of radical thought and more consideration of the institutions that have felt the handbag (e.g., the legal, medical, and educational professions in the final administration).

The Dunshire and Hood volume is a major, grant-supported work attempting to test hypotheses about cutback management. The chapter introductions are trenchant and witty for such a heavy volume of data, and there are many nuggets among the hypotheses, although tracking the supportive findings requires frequent cross-referencing. The authors themselves properly point to limitations of the official data: voluminous dross, rather than a few solid cases.

One of the themes is the deep retrenchment in the sheer number of civil servants, which they properly locate in 1976–85, beginning under the International Monetary Fund–influenced Labour government of 1976–79. This 20% reduction, they point out, is the third greatest in the twentieth century (after Geddes' Axe of the Depression and the demobilization following 1945). Even then, a third of the recent cuts occurred in factory privatization, rather different from the earlier deep cuts in nonindustrial white-collar positions.

Their examination of the effects of bureaucratic vulnerability provides a welcome extension of theoretical work by Charles Levine and by T. Beck Jorgensen (among others) and a particularly welcome empirical testing. Their amusing trialogue between credos on the consequences of bureaucratic slimming adopts Leaner Means Fitter as the official religion, with Leaner Means Anorexia as the commonest dissent. The third (*gatto partismo*) implies that a leopard cannot change his spots.

The researchers are sensitive to secular economic trends: the loss of blue-collar jobs to electronic transfers and automation merely found its political expression, not its driving force, in Thatcherism. They argue that the scope for major cuts has been taken already but that a plate-tectonic shift of popular expectations will keep the contract state rather than the direct service state as the dominant model. They leave open the possibility of a future government finding a higher plateau of direct services. Generally, Dunshire and Hood enrich our debate over the consequences of Thatcherism considerably.

Butcher and his colleagues are concerned with "radical consumerist reforms," or "surrogate market mechanisms" introduced into local government but not blinded to the "arithmetic politics" of pluralism. In the process, the authors are keenly aware of the paradox of proactivity to roll back the state; they evaluate the process as highly disintegrating and partisan, an aspect that is perhaps underemphasized in the other two volumes. It may have been a theory of public choice that led to the poll tax, but its effects were both partisan and particularistic. They consider the challenge to local government not only a recasting of the social order (as would Haseler) but also as refashioning the local political process. They find three models of local political authority in the 1990s: *contract*, embracing both changes of order and process; *business–corporatist*, combining collectivist (economic development) values with particular political interests; and *enterprising* (adoption of user fees to expand services), which is antistatist but serving particular interests.

All three volumes should find a place on our shelves: Haseler for the intelligent center of scholars and persons; Dunshire and Hood's and Butcher and his colleagues' volumes are both treasure troves for public administration scholars. Haseler is rather laudatory of the Thatcher administrations, Dunshire and Hood less so, and Butcher and his colleagues more critical. One specter they all detect is that the central models of administration in the 1990s will be heavily influenced by the popular, plate-tectonic shift harnessed by Thatcherism. The two data-driven volumes also indicate that the Thatcher administrations have not removed the value of official statistics.

University of Northern Iowa JEREMY R. T. LEWIS

Administrative Argument. By Christopher Hood and Michael Jackson. Aldershot, UK: Dartmouth, 1991. 221p. $49.95.

Over 40 years ago, Herbert Simon denounced much of the conventional wisdom of public administration, calling most of the field's popular precepts mere "proverbs" lacking substantial theoretical foundation or systematic empirical support. Simon called on scholars interested in administrative matters to apply the rigorous tools of "normal science" to those proverbs, in order to separate fact from fiction and thus improve the field. Today Christopher Hood and Michael Jackson conclude that the research program that Simon proposed and in which he has long participated has failed to eliminate many of the traditional proverbs from the lexicon of administrative theory. This is not because of the merit of these proverbs, according to Hood and Jackson, but because their persuasiveness is not based upon normal science. In light of this, Hood and Jackson choose not to examine the validity of administrative arguments but to describe, compare, and catalogue them in an attempt to understand their acceptance and resilience. In doing so, Hood and Jackson try to explain how and why certain administrative doctrines are adopted in certain places at certain times.

Almost half of the book consists of short descriptions of 99 doctrines gleaned from the literature. Each is discussed in terms of different kinds of justifications, including sigma-type (representing a concern for efficiency), theta-type (concerning fairness and accountability), and lambda-type (ensuring reliability, robustness, and adaptability). In examining three historical case studies of the adoption of administrative doctrines (the British Northcote–Trevelyan Report of 1854, the U.S. Brownlow Report of 1937, and the New South Wales, Australia Wilenski Reports of 1977 and 1982) Hood and Jackson identify six attributes that constitute an "acceptance factor." These attributes concern the rhetoric, presentation, and timeliness of the doctrines rather than

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their internal content. Hood and Jackson claim that many in mainstream public administration fail to appreciate the rhetorical, symbolic, and metaphorical importance of their ideas and that this has limited the extent to which their advice has been sought and their ideas accepted.

The authors are quite successful in making a case for the rhetorical importance of administrative argument. They have also provided readers a handy summary of many diverse and often contradictory ideas about administration. Political scientists, however, will probably be disappointed that Hood and Jackson put little emphasis upon the role that interested parties have in urging the adoption or rejection of particular doctrines. The substantive impact that the implementation of doctrines may have on politically important interests is given little systematic treatment. This will be hard for many readers to understand.

Another difficulty concerns the rationale behind the components of Hood and Jackson's "acceptance factor." These attributes are said to fit the three cases examined in the book; but (as the authors remind us) advocates for doctrines tend to be selective about their choice of evidence. Perhaps Hood and Jackson are similarly selective about what they report. Certainly, their "doctrines" about persuasiveness at this point are little more than proverbs, lacking much evidence or theoretical underpinning. Only time will tell whether their ideas will be accepted.

Texas A&M University JAMES C. CLINGERMAYER

The State of Martial Rule: The Origins of Pakistan's Political Economy of Defence. By Ayesha Jalal. Cambridge: Cambridge University Press, 1990. 362p. $44.50.

"Midnight's children"—India and Pakistan—share a common history. Both were born out of an undivided British India in 1947. That Muslims and Hindus were not simply two separate religious communities but also two distinct nations was the argument of Mohammed Ali Jinnah, father of Pakistan and its first head of state. Of the nearly 100 million Muslims of undivided India, constituting one-fourth of the country's population, 65 million became citizens of Pakistan. About 35 million were left behind.

Whatever happiness a Muslim homeland created in 1947 was soon gone. Jinnah died within a year of the partition. Liaquat Ali Khan, second in command, was murdered two years later. Kashmir, a Muslim-majority state, did not join Pakistan; and a war with India did not settle the issue. Pakistan could not even formalize a constitution until 1956; and in 1958 the military stepped in with a martial law regime. The *first* national elections took place as late as 1970; and the bliss of their first free elections soon turned into the nightmare of yet another partition as East Pakistan became Bangladesh, decimating the two-nation theory and shattering Pakistan's army in a war with India. A shattered army did give way to civilian rule; but eight years later, Prime Minister Bhutto was hanged by a new military ruler, who was himself killed in a place crash in August 1988. A shaky democracy has been operating since then. Pakistan has lived under six constitutions and the military has ruled for almost half of the country's life. In contrast, with a brief exception, India has managed to retain a democratic political system which, though weakened over the last 15 years, has survived elite misbehavior, political assassinations, and mass violence.

Why has Pakistan been largely authoritarian, ruled by a military–bureaucratic coalition? This question is at the heart of *The State of Martial Rule.* Pakistan's unending political bunglings and misfortune placed on its scholars a painful burden of contrast with its neighbor. Moreover, dissent and critique are not always appreciated in authoritarian systems. If India had gone through similar tragedies or had crushed freedom of expression, it would have been very difficult to be a self-respecting political scientist or historian in India. For a long time, only one Pakistani scholar writing on the country's politics, Khalid bin Sayeed, was able to meet international standards. Others either engaged in diatribes written from faraway diasporas (e.g., Tariq Ali) or, if left in the homeland, wrote ill-constructed valentines and laments. Happily, this situation has started changing. Over the last decade, some notable commitments of intelligence and candor have been in evidence. Ayesha Jalal belongs to this new generation of scholars.

To understand Pakistan's authoritarian post-1947 history, she argues, one has to investigate the nation's troubled childhood when a civilian experiment was tried out. Four factors militated against democratic rule. First, the Muslim League (the party leading the movement for Pakistan) had virtually no organizational base in the land mass that became Pakistan. The league was strong in Muslim-*minority* areas of British India, whereas Pakistan was created in the Muslim-*majority* areas. (India was blessed with the huge, nationwide organizational machinery of the Congress party.) Second, Pakistan had to construct not only a party organization but also a state. The state machinery of British India was lodged in Delhi. Pakistan's government was "without any ground under its feet or a roof over its head" (p. 29). Its share of spoils from British India also left it abysmally poor. Third, Pakistani Muslims may have shared a religion; but they came in varying ethnic, linguistic, and cultural hues. The provincial pulls began to overpower any cement that Islam might have provided. Fourth, Pakistan felt a hostile regional environment—India on one side and Afghanistan on the other. A war with India over Kashmir almost as soon as the nation was born stressed the seriousness of the defence threat.

What was the overall result? Lacking a well-organized political party and faced with centrifugal internal forces and external threats, Pakistani leaders—even the best of them—concentrated on state building, not party building. The state had to survive and be coherent first; party building was a long-run task. Overpowering short-run imperatives led to the eclipse of elected institutions and the rise of state institutions. Once in place and bolstered by powerful international allies like the United States, (which found in Pakistan a Cold War ally) the military–bureaucratic coalition developed structural hegemony in the system.

Painstakingly researched and elegantly written, Jalal's book adds to our knowledge. Her analysis of how ill-equipped the state was in its childhood forces us to qualify substantially the conventional wisdom that the historical inheritance of India and Pakistan was identical. Only in the ideational sense was the British legacy similar. Institutional legacies had important differences.

Still, one is tempted to ask whether *both* party building

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and state building could not have been undertaken at the same time by Pakistan's founding elite. Comparative literature on democratization suggests that leaders in their moments of popular legitimacy (the "honeymoon period") can act on their visions and undertake difficult initiatives. After independence, leaders of national movements enjoy enormous legitimacy, which gives them room for maneuver: they are not entirely prisoners of their circumstances. Were Jinnah and his colleagues party builders at all? How important was party building in their view of Pakistan? Jinnah's politics suggests that he was a brilliant constitutionalist but not a mass politician or a party builder. Even before independence when times were less unkind, Jinnah did not organize the masses like Gandhi and Nehru did. Had a nationwide Muslim League ever been built, there would have been no organizational vacuum in postpartition Pakistan in the first place. In addition, right after the breakup of the country in 1971, political leaders had yet another opportunity to build a party, since the defeat in Bangladesh had devastated the military.

Jalal gives us a powerful account of how the politics was, rather than how it might have been different. To be sure, it is not easy to deal with counterfactuals rigorously; but if alternative routes were possible, the burden of explanation would have to shift from structural difficulties to a fault of vision. It may well be that Pakistan's political future was "insufficiently imagined."

Harvard University ASHUTOSH VARSHNEY

The Dialectic of Change. By Boris Kagarlitsky. New York: Verso, 1990. 393p. $69.00 cloth, $22.95 paper.

The merit of this book is that it considers the current strategy and theory of socialism in a comparative and interdependent perspective: "*The triumph of the Left in the West is inconceivable without changes in the developing countries and in Eastern Europe* and the political struggle which is happening there is subject to the same general dialectic of reform and revolution" (p. 151; emphasis original). The author, currently executive committee member of the Russian Socialist party, seeks to reconstruct Marxism; and the major theme of the book is that reform is now universally the appropriate tactic for the socialist Left.

This viewpoint is substantiated with reference to Western Europe (particularly Britain), Eastern Europe (specifically Poland), and the USSR. The book is a collection of essays around a general theme. It is well referenced to literature in several languages and the argument is buttressed with pertinent examples; however, it is repetitive and rambling in places. The "dialectic of reform" searches for a social base to underpin the various revolutionary and reformist wings of contemporary workers' and socialist movements. The author condemns the politics of the "old working class traditionalists" in Britain (clustered in the trade unions and the Left of the Labour party), on the Continent (the Eurostalinists in various unreformed Communist parties), and in the East (the Brezhnev wing of the Communist party if the Soviet Union).

Kagarlitsky condemns their revolutionary stance and rebukes their leftist anti-reformism as a protest against technical progress, precluding political change and entailing retrogressive economic protectionism and autarky. This is a somewhat uncharitable representation of the Left: for instance, the British Labour party opposition to membership in the European Economic Community was not reactionary but was based on the consideration that socialist political policy would be inhibited by an enlargement and strengthening of the economic market and would minimize change through political control of the state. Students of British politics will find Kagarlitsky's argument interesting but faulty. There is little discussion, for instance, of the growing influence of feminist and ethnic politics on the Left. The more radical attempts by the Left to use local government organs to secure socialist policies (such as Ken Livingstone's leadership of the Greater London Council) are also ignored. While advocating socialist reformism through the "new working class," Kagarlitsky ignores the major structural change and dealignment in British politics articulated in the social democratic movement led by Williams, Rodgers, Jenkins and Owen. Such social democracy, like the Swedish kind, does not fit with the author's political viewpoint but must be considered to be one possible outcome of the structural analysis advocated here.

In reviewing changes in the Soviet Union, Kagarlitsky takes the side of Western Sovietologists such as Hough, Lewin, and Ruble (though they are not mentioned by name) in focusing on the social consequences for politics of the changes in class structure that took place under Brezhnev. Unlike the majority of Soviet reformers, the author rejects totalitarianism to describe Soviet politics and adopts an interest group approach. The Brezhnev period, he concludes, was "undoubtedly one of the most prosperous and successful periods in Soviet history" (p. 339). The conservative basis of Brezhnev's politics was an alliance of the old working class and the entrenched bureaucracy, whereas the reformers had a social base among the young, skilled manual workers and qualified non-manual employees. This political cleavage provides the social dialectic of change in the Soviet Union. The chapters on contemporary Soviet politics are well referenced, informative, and make useful reading for upper-level classes on Soviet government courses.

What, then, is Kagarlitsky's vision of the contemporary socialist project? He insists that socialism cannot exist without political democracy, workers' self-management of production, nationalization and planning, and market relations in the economy (p. 14). In discussing the inadequacy of the Polish leadership of Solidarity, the author revives interest in the notion of a "political vanguard": it is "impossible to do completely without a strategic vanguard" (p. 207). The discussion, however, is insufficiently theorized and inadequate as policy. How are nationalization and planning to be reconciled with workers' management? And what form does the latter take? Kagarlitsky warns against the "fetishism of the market." But where does fetishism depart from consumer need fulfillment? For a self-styled Marxist analysis, there is, surprisingly, no discussion of the market as a system of political power. How does the "socialist market" overcome the private appropriation of surplus value? And if consumers' preferences are to direct investment through the market, what is the political capability of socialist planning? There is no discussion of why state ownership in the form of nationalization is preferable in allocation and efficiency terms to private ownership.

Kagarlitsky's book is interesting and will provoke

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discussion in circles concerned with reconstructing Marxism. What remains unclear is just how much, if anything, of the theoretical underpinning of classical Marxism theory of value, classes, revolution is to be retained and how it informs contemporary socialist strategy. Most, if not all, of Kagarlitsky's conclusions would be shared in varying degrees by many non-Marxists and even anti-Marxists (e.g., C. A. R. Crosland, who is not mentioned in the book). Though there is little new here for the specialist on the societies described, Kagarlitsky's comparative approach and sociological insight have novel features that single out his book as one of the best on contemporary European society to have emanated from the contemporary Soviet Union.

University of Cambridge DAVID LANE

Everyday Politics in the Philippines: Class and Status Relations in a Central Luzon Village. By Benedict J. Tria Kerkvliet. Berkeley: University of California Press, 1990. 326p. $45.00 cloth, $13.95 paper.

Until not so long ago, the literature on peasant politics tended to focus either on bold, mass rebellion or on meek, acquiescent subordination. The new "everyday forms of resistance" literature transcends that dichotomy, broadening and deepening our understanding of political behavior. *Everyday Politics in the Philippines* is an excellent complement to James Scott's now classic *Weapons of the Weak* (1985). The two also coedited *Everyday Forms of Peasant Resistance in Southeast Asia* (1986).

Kerkvliet's *Everyday Politics in the Philippines* is of significant interest far beyond an area studies audience, since it addresses fundamental questions about how subordinated people understand the power relations in which they are enmeshed. Political values and preferences cannot be assumed or imputed from "public" actions or socioeconomic position alone. Kerkvliet brilliantly demonstrates that ideas about justice matter a great deal to those on the brink of survival. Based on 17 years of intimate ethnographic village study, he shows how rural workers and peasants act politically based on strongly held normative values, though always constrained by their available resources, as well as perceived risks and opportunities.

The conventional approach to rural politics in the Philippines highlights vertical patron–client relations and political factions, complicated by religion and kinship. These institutions are often associated with a harmonious political culture, celebrated by ritualistic electoral "fiestas." This certainly captures part of the picture: the rural poor do seek out patrons, and elections in the Philippines are rarely the arena for class- and issue-based contestation. Yet there are several problems with this approach. It cannot account for repeated waves of mass, class-based political mobilization, sometimes nonviolent, sometimes armed. Moreover, the "political faction" approach assumes that when rural workers and peasants are not in open rebellion, they are therefore acquiescing to sharp class and status differences. This study shows, in contrast, that the rich clearly do not have ideological hegemony.

For Kerkvliet, "politics consists of the debates, conflicts, decisions, and cooperation among individuals, groups, and organizations regarding the control, allocation and use of resources and the values and ideas underlying those activities" (p. 11). For most of the villagers during the "war era" (1941–52), this meant active support for the anti-Japanese resistance and later the Huk rebels. (See Kerkvliet's definitive *Huk Rebellion*, 1977.) The government responded with tenancy reforms that the villagers considered meaningful; but, since then the increased capitalization of agriculture sharply aggravated landlessness. Most poor people feel worse off, and they certainly do not blame themselves. This does not mean that they have turned to overt opposition to the government, however. Kerkvliet reports that the armed revolutionary movement that swept much of the upland countryside in the late 1970s and early 1980s has not made much headway in this central lowland region.

In everyday village politics, the political discourse of the majority strongly upholds rights to both security and dignity (although in practice they may conflict). Among the employers, landlords, moneylenders, and traders who most directly shape the terms of the majority's existence, some are seen as more unfair than others. Apparently small differences turn out to matter a great deal, such as the quality of the midday snack employers provide to rice transplanters. (Each employer is expected to provide according to their ability, but the poorest turn out to be the most generous.) Elites who treat people with more dignity are sought out as patrons, while those who are disrespectful provoke reciprocal treatment: pilfering of harvests, stealing of tools, rocks thrown in the middle of the night. Most everyday resistance is angrily spontaneous, but not all is individual. Mass collective action is chosen when people unite around common grievances that seem "winnable" and the government is seen as relatively unlikely to intervene.

The book convincingly argues that for most people most of the time politics is informal and stays within the village. Yet we also learn that some villagers repeatedly participate in regional and national movements to increase peasant representation and accountability in the political system as a whole, usually through nonviolent mass movements—first in the 1930s, then in 1946, and again in the late 1960s. In spite of martial law, the Association of Central Luzon Farmers emerged in 1981 to lead smallholder and landless movements in the region. The organization is clearly very important to villagers (pp. 141–43), but we learn relatively little about how it relates to the community. Kerkvliet readily acknowledges that villagers see local leaders and larger organizational allies as quite important but does not focus on their dynamics. Where are the "everyday politics of organization building?" To move beyond everyday resistance to understand the limits and possibilities for the broader democratization of the countryside, we will need to learn much more about how the organizations of the rural poor manage to survive and grow in such inhospitable environments.

Massachusetts Institute of Technology JONATHAN FOX

Multiparty Government: The Politics of Coalition in Europe. By Michael Laver and Norman Schofield. Oxford: Oxford University Press, 1990. 308p. $59.00.

Michael Laver and Norman Schofield offer us the first comprehensive survey of coalition government in nearly two decades. In their preface, the authors claim

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that their most important objective is to "impart a real 'feel' for what is involved in the fascinating process of coalition bargaining" (p. vii). To say that they have succeeded would be an understatement. Laver and Schofield's book is at the same time accessible, pithy, down-to-earth, insightful, and, yes, even captivating. In its scope and quality, it has no rival in the literature on party coalitions.

The authors distinguish between two traditions in the study of coalition politics: the European politics tradition and game theory. The first label appears to represent mainly cross-national analyses of a conventional institutionalist bent. Somewhat surprisingly, this is the context in which Laver and Schofield place their own book. Yet their main concern is to help bridge the gulf that has developed between the two traditions.

In their first substantive chapter, Laver and Schofield discuss the conventional conception of political parties as unitary actors in coalition bargaining. They proceed to examine, successively, party objectives, the process by which governments are formed and the requirements they must meet, coalition membership, cabinet stability, coalition payoffs, and, finally, the affects of institutional and other constraints on coalition bargaining.

Each chapter provides a crisp, fair, and comprehensive survey of the literature in its field. In addition, some chapters present new data or launch novel theoretical departures. All chapters are broad-ranging and well written, but some are more innovative than others. Thus, the conception of parties as unitary actors comes under much more probing examination here than anywhere else in the literature; the process of cabinet formation and investiture is laid out in unusual institutional detail; multidimensional models of coalition bargaining are discussed at length and confronted with new data; and structural constraints on coalition bargaining are for the first time discussed systematically. In each of these areas, Laver and Schofield's book makes a significant contribution. Other chapters, such as the discussion of cabinet stability, are less original and therefore also less interesting to the initiated reader.

Among the greatest virtues of Laver and Schofield's book are the clarity of its arguments and the elegance of its prose. No reader will feel intimidated by the authors' conceptual apparatus or notation. And Laver and Schofield's prose is refreshingly irreverent and down-to-earth. Consider, for example, the following discussion of the lure of different cabinet portfolios:

> "The Prime Minister, after all, tends to get far more of the limelight, to consume far more dinners in interesting and exotic places and to be driven around in the back seats of bigger cars than other members of the government. . . . Other government posts may have their own attractions. The Minister for Foreign Affairs, for example, gets to travel widely, to gather quite a lot of the limelight and to risk less of the odium that attaches, say, to a Minister for Social Welfare forced to cut old age pensions in a budget crisis" (p. 40).

The research frontiers of coalition studies are being moved rapidly these days. As a vehicle for readers who wish to reach those frontiers with speed and in comfort, Laver and Schofield's book has no competition. Yet in some areas the authors could only begin to address issues that may transform the study of party coalitions. The effects of intraparty politics and institutional structure will doubtless receive much more intensive scrutiny in the years ahead, not least from adherents of the "new institutionalism." The assumption that political parties are driven by policy concerns, that is by the pursuit of largely public goods, will ultimately need more rigorous defense. In the meantime, all students of political parties or legislative behavior should allow themselves the privilege of being educated and entertained by Laver and Schofield's fine book.

University of California, San Diego KAARE STROM

Hawke's Law: The Politics of Mining and Aboriginal Land Rights in Australia. By Ronald T. Libby. University Park: Penn State Press, 1989. 208p. $16.95 paper.

Libby's account of the rise and fall of national legislation to expand aboriginal land rights and control of mining in Australia during the first period of Australian Labor Party (ALP) government under Prime Minister Bob Hawke (1983–87) contrasts with dependency theory and "privileged position" interest group explanations of the mining industry's influence on Australian public policy. In his view, these explanations fail to describe what actually transpired. The withdrawals of national land rights legislation and the mining industry's influence in that withdrawal were conditioned (in places he writes "determined") by factional politics within the ALP.

Libby's book is strongest when he takes on prevailing explanations attributing Hawke's decision to drop national land rights legislation to the mining industry's economic strength, without regard to its strategy for fighting the legislation or the political context in which it was pursued. He demonstrates that once the ALP came to power, the relatively resourceless aboriginals became insiders in the process of drafting the legislation, while the mining industry was not even consulted. The mining industry fought the legislation with classic outsider tactics, mobilizing public opinion (already disposed against additional concessions to aboriginals) through a publicity campaign in the state that would be most affected, Western Australia. Followers of water battles in the U.S. West may find intriguing parallels with the contests between irrigated agriculture on the one hand and groups pressing environmental concerns and Indian water rights claims on the other. For those interested in policy formation, Libby's descriptions of the legislation-drafting process and the Western Australia miners' campaign are interesting and useful, illustrating how the political context affects the strategies political actors select and their prospects of success.

Libby's documentation of that political context is another strength of the book. Premier Brian Burke's Labor government in Western Australia rested on shaky electoral ground: Labor had succeeded the Liberal party in state government by winning seats in the mining and pastoral districts that normally vote Liberal and whose voters strongly opposed land rights legislation. Labor seats from Western Australia, in turn, provided the crucial margin in the national parliament; Labor losses in Western Australia could topple Hawke's government. Burke led an ALP faction that revolted against the national party's platform in favor of land rights legislation that would relinquish important aspects of Crown control of mining to the aboriginals. Hawke responded by supporting Burke's position, even though the plat-

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form had been adopted unanimously at the biennial national conference just three months earlier.

Perhaps ironically, the information Libby provides about the political context weakens the support for his conclusion that ALP factional politics determined the mining industry's influence, rather than the other way around. His conclusion reflects his choice about what to regard as *context* and what to regard as *action*, but his choice is not always persuasive. He contends that the Burke–initiated factional split within the ALP was the context within which the mining industry acted; but there is at least as much support in the book for the view that mining's importance in Western Australia, the recognition of that importance by voters, and the industry's campaign to publicize voter support for the mining industry and opposition to land rights legislation constituted the context within which Burke decided to reject the national party platform on aboriginal land rights.

Libby diminishes the mining industry's role in Burke's decision by showing that public opinion already opposed expanding aboriginal rights and that the industry's advertising campaign strengthened that opposition only marginally (p. 82). However, the same poll data show that 87% of Western Australians recognized favorably the mining industry's contribution to the state's economy (a year after the conclusion of the industry's campaign to promote itself as the "backbone of the nation") and other poll data reveal that the strongest opposition to land rights legislation was in the mining districts Burke was in greatest danger of losing (p. 93). Libby points out that the Liberal party mounted a vigorous challenge to Burke during the period and concludes from this that the mining industry's role was smaller, but notes elsewhere that the mining industry had historically been aligned with the Liberal party. Burke threw the party platform overboard, brought the mining industry into the process of drafting land rights legislation for Western Australia that was more to the industry's liking, defused the land rights issue for the Liberal party, and got himself and Labor reelected a year later. Libby is persuaded by all of this that the mining industry's campaign was less successful and its influence less strong than commonly believed. Readers may not be quite as persuaded.

Another conclusion seems inadequately supported by the evidence Libby presents. Although it dropped national land rights legislation in 1986, the Hawke government failed to go farther and remove the aboriginal veto over mining from the 1976 land rights law governing the Northern Territory. Libby describes this as a "setback" for the miners (p. 153). In 1984 the industry faced a Labor government unanimously in support of national aboriginal land rights legislation. In February 1986 miners emerged no worse-off than they had been before Labor was elected; the status quo ante had been restored. The fact that the ALP did not change existing law to improve the situation of an industry that historically supported its opposition might not be appropriately characterized as a setback.

Students of comparative federalism will find this book intriguing. Although it is beyond Libby's purpose to do so, the fight over land rights legislation could be fashioned into an interesting story in intergovernmental relations as well. Objections to national legislation had a states' rights dimension that the Liberal party employed to gain support in Western Australia and others denounced as having racist undertones. Something about that seems familiar.

Indiana University, Indianapolis WILLIAM BLOMQUIST

Social Justice for Women: The International Labor Organization and Women. By Carol Riegelman Lubin and Anne Winslow. Durham: Duke University Press, 1990. 328p. $45.00.

Women, International Development, and Politics: The Bureaucratic Mire. Edited by Kathleen Staudt. Philadelphia: Temple University Press, 1990. 320p. $39.95.

Bureaucracies have remained paramount to how societies function, and their behavior frequently reveals contradictions within the societies themselves. It is no surprise that as women in advanced capitalist societies have joined the ranks of bureaucracies, the "bureaucratic mire" in which they find themselves has a particular substance and character. Lubin and Winslow and 15 contributors to Staudt's edited volume wade through this mire to examine how bureaucracies have treated women who are part of their organizations. Contributors to the Staudt volume evaluate the success of bureaucracies in designing and implementing development projects targeted for women; Lubin and Winslow trace the struggles to enact international labor laws for women. Both volumes explore the effectiveness of women's politics within and outside bureaucracies in relation to the development of programs and policies that address women's needs, the institutional and political contexts in which international and national bureaucratic agencies operate, and the socioeconomic dynamics of the agencies themselves.

With rare exceptions, the conclusions reached are the same: women have been able to make few inroads within or outside bureaucracies in bringing about changes that would benefit women. The reasons for this "grim scenario" (as Staudt herself describes it, p. 8) are not surprising. Existing patriarchal structures and attitudes prevent women from being taken seriously within agencies. Few women, therefore, reach positions of decisionmaking in which they can formulate or implement projects targeting the improvement of women's status. Women often internalize patriarchal attitudes and behaviors. Those that do are more likely to achieve more powerful positions within bureaucratic agencies. They often attach little importance to precisely the policies that women resisting patriarchal structures would like to see implemented. Occasionally, men committed to incorporating women into more autonomous positions of policy design and project implementation come to occupy positions of great power within bureaucracies but are eventually ousted by board members or governmental authorities threatened by their radical stances and the slow success rate of development projects for women.

The slow success rate of development projects for women can be attributed to a number of factors: bureaucracies or societies within recipient countries have their own patriarchal ax to grind; class differences among women exist within host and recipient countries so that women's programs often exclude most women or confine them to traditional domestic roles; so few funds are channeled toward these projects that they are likely to be doomed to failure; and an essential conflict exists

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within host and recipient countries about how monies can be used best. Most policymakers believe that the dire circumstances of many countries make it more important to channel funds into general development projects that could affect greater numbers of people. On the other hand, those concerned with incorporating women into development projects point out that general development projects frequently fail because women have not been taken into account and that women will never be able to transform their subordinate position unless projects are geared to enhance their self-esteem and economic and political power. Hygiene, nutrition, marketing, and agriculture are but a few areas in which women play central roles but are often completely ignored in the design and implementation of policy.

Women shoulder greater economic responsibility throughout the world due to the rising rate of rural migration to urban areas, including cities in other countries, and the shrinking base of agricultural subsistence. Nevertheless, the authors' focus on the bureaucratic nexus as the place to examine issues is far too narrow. The double jeopardy of trying to design new policies within Western-style bureaucracies and then implementing them in multiethnic recipient countries whose bureaucracies and cultures may be vastly different in their structure and operation has always stymied the successful implementation of projects and the effectiveness of international laws. No doubt women experience greater subordination and frustrations within bureaucracies due to existing gender relations. However, bureaucracies themselves are responsible for the failure of many of the projects described. Innovative ideas are rarely accepted or welcomed by bureaucracies. The kinds of conflicts these authors describe are typical of any bureaucracy and can involve men as much as women. Occupying middle management positions means occupying contradictory locations of great tension in which implementation of normative policy is permissible, but provides little control or power to introduce new ideas or enforce one's point of view. Bureaucracies have never been autonomous, although they may be pervasive and dominate public life. Bureaucratic policies are frequently linked to prevailing political ideologies and economic policies (particularly those of more powerful governments) and the demands of national or international capital.

In the Staudt volume, Cathy Small's article is exceptional in the thoughtfulness and subtlety of its analysis. Writing on the development of the Moving Forward Organization, a women's self-help organization that redistributed village wealth into the hands of women in the Kingdom of Tonga, South Pacific, she concludes that the success of the project, administered by a bureaucracy largely in the hands of women, was due to the nature of existing indigenous social structures and processes and that the role of the bureaucracy was comparatively irrelevant. Women have always occupied a special place in Tongan kinship hierarchies. Their status permitted them to extract economic obligations including property from men, activate marriage alliances, and played a determinant role in "influencing power and claims to titles" (p. 270), including that of chiefs: "Because of these often complex social dynamics, not only women, but all Tongans had a real investment in the superior status of women as sisters" (p. 270). Small stresses that, "In the end, it is these historical conditions and indigenous dynamics—rather than the structure or content of the development enterprise—that underlie the popularity, the character, and the success" of the Moving Forward Organization (p. 279).

Small's article leads one to question the notion of development itself, as does Kathy E. Ferguson in "Women, Feminism, and Development." Ferguson summarizes my objections to the major currents that run through the Staudt volume. It is a credit to Staudt that she includes Ferguson's perspective. Ferguson points out that tremendous tension exists among the contributors to the volume between concern with the equitable distribution of the goods of development and with development programs' often acting to the disadvantage of women and concerns about development itself—who benefits from it and who is excluded, silenced, or controlled. In the former instance, an overwhelming degree of attention is paid to development agencies as primary actors, and to development as "something that the aid bureaucracies *do* to the local people" (p. 299). Ferguson recommends a need to shift control to local communities and to question the kind of control exerted by the development agencies and process. She concludes that although local communities are not virtuous and have their own patriarchal biases, "moral and political priority in making decisions about development should reside with those whose lives are most directly affected by these decisions" and that "self-empowerment of local people" should be a primary goal (p. 299). In these terms, development would take on a more politicized character in which attention would focus not so much on aid agencies but, rather, on the goals of local peoples, including the indigenous struggles of women. She also squarely confronts the problem of women working in bureaucratic agencies and recommends, in line with her thinking about development itself, that these women should be perhaps more engaged in changing their own social structure, forming alliances outside the bureaucracy, establishing grass-roots links with local constituencies and activists, and questioning their own government's roles in maintaining global inequities.

Lubin, a veteran of the International Labor Organization (ILO), and Winslow trace the history of women's participation in British and North American trade union movements and their roles in the founding and development of the ILO. The book dramatically contrasts the efforts made, mostly by women, to improve the status of women workers through the use of the ILO, with the relatively few gains that were actually achieved in the interest of women workers. The authors' painstaking research of the history of women's participation in affecting ILO policies is admirable. However, the book lacks a theoretical orientation that would unify these nuggets of information. The task of discovering who these women were was daunting and the book's structure attests to the problem scholars encounter who wish to revise historical accounts to include women who were significant actors. The book will nevertheless primarily serve as a reference. We are left only with blow-by-blow accounts of debates and a grasp of the issues at stake in each debate. The stark contrast the authors draw between the nature of modern feminism among Western women and traditional machismo among women from non-Western or less-developed countries is far too general and oversimplified, although, unfortunately, it permeates many works on gender relations in society.

Both books find their strength in taking traditional international organizations and policies and reevaluat-

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ing them in terms of gender relations. However, both suffer from overemphasizing the category of women and losing sight of the social, institutional, historical, and political context in which these gendered relationships unfold. Staudt's volume includes selections useful for classes on development and issues of gender; Lubin and Winslow's volume will be a valuable resource for labor historians.

University of Wisconsin, Madison LINDA J. SELIGMANN

Social Policy and the Welfare State in Sweden. By Sven E. Olsson. Stockholm: Swedish Institute for Social Research, 1990. 238p.

The Political Theory of Swedish Social Democracy: Through the Welfare State to Socialism. By Tim Tilton. Oxford: Clarendon, 1990. 280p. $39.95.

The Swedish Experiment in Family Politics: The Myrdals and the Interwar Population Crisis. By Allan Carlson. New Brunswick: Transaction, 1990. 235p. $39.95.

Suppose that we could measure the attention that foreign scholars have paid to the domestic arrangements of different countries and that we deflated this measurement by population size: my bet is that Sweden would rank first on such an index. Presumably, it is Sweden's success in reconciling economic efficiency and social equality that accounts for its notoriety. Sluggish productivity growth and widening social inequalities in the last 15 years raise questions about the durability of this historic achievement. Perhaps it has become more difficult for Sweden and other countries to reconcile efficiency and equality? However, these recent additions to the literature on the Swedish experience remain within the traditional problematic of comparative welfare politics. Leaving the conditions of capitalist growth aside, they ask: Where has the impetus behind the development of the welfare state come from? and Why have reformist forces prevailed in Sweden?

While Sven E. Olsson seeks a comprehensive account that synthesizes different arguments advanced by students of comparative welfare politics, Tim Tilton seeks to break new ground by emphasizing the importance of ideas of the Swedish Social Democrats. For Olsson and Tilton alike, the development of the Swedish welfare state is inextricably linked to the political dominance of social democracy. Allan Carlson's more narrowly conceived study of the intellectual origins of Swedish family policy seconds Tilton's emphasis on ideas; but for Carlson, the prime movers are intellectuals who stand outside the ideological mainstream of the Labor movement.

Olsson provides the most careful and comprehensive presentation of the postwar development of the Swedish welfare state available in English (perhaps in any language). The analytical significance of his descriptive analysis is to emphasize the composite and multifaceted character of the Swedish welfare state. Olsson shows that the Swedish welfare state combines selective state intervention in labor markets and means-tested social assistance with universalistic welfare programs, social insurance with socialized consumption, and income maintenance with income redistribution and periodizes its development in terms of the introduction of new elements or changes in the relative importance of different elements.

This complexity leads Olsson to argue for a multivariate explanatory approach, emphasizing the interaction of demographic, economic, and political factors. In key passages, Olsson asserts the central importance of politics. Except for the chapter on the pension reform of 1946, however, his book contains precious little discussion of the politics of policy formulation and legislation. For Olsson, "politics" boils down to the balance of power among class actors (pp. 249–50). Though I share his preoccupation with class power, I would argue that Olsson's approach does not adequately address the "translation" of class power into policy outcomes.

Through a careful and insightful analysis of the writings and speeches of the major intellectual figures of Swedish social democracy, Tilton successfully refutes the unattributed but allegedly common (p. 40) assumption that the Swedish Social Democrats lack a coherent ideological foundation. His case for the political significance of ideas and ideology is less compelling. Tilton argues that ideas serve to guide strategic political action, to enhance political support through ideological appeals, and to generate concrete policy initiatives. Though he suggests that postwar welfare state activities broadly correspond to social democratic ideas, he never engages in a systematic analysis of how, where, and when certain ideas come to be translated into (more or less successful) strategic choices, ideological appeals, or policies.

Tilton asserts that "structural explanations . . . fail to do more than outline the general potential for reformist politics" (p. 9), but he does not specify what it is that structural explanations cannot account for. Furthermore, the inadequacy of structural explanations does not in itself vindicate Tilton's history-of-ideas approach, for there are plenty of other ways to bring human agency into the explanation. Most importantly perhaps, Tilton never addresses how the ideology of Swedish social democracy differs from that of other reformist Labor movements in Western Europe. If all reformist Labor movements adhere to the notion of a "socially controlled market economy," for instance, this notion can hardly be invoked to explain the political success of the Swedish social democrats. There is no question that the Swedish Labor movement has been shown to have a remarkable capacity to generate innovative policy proposals; the question is how this capacity of policy innovation is related to the basic elements of social democratic ideology identified by Tilton.

Carlson's detailed study of the intellectual context and political impact of Alva and Gunnar Myrdal's *Crisis in the Population Question* (1935) effectively probes the role of intellectuals and ideas. Carlson shows how the Myrdals wrested the population issue away from conservative forces (linking the need to reverse the population decline of the previous three decades to neo-Malthusian concepts of voluntary parenthood, sex education, and birth control, as well as the feminist theme of women's full participation in the labor market) and demonstrates how their reformulation of the terms of debate led the leadership of the Social Democratic party to adopt a pro-natalist position. He proceeds to describe the Myrdals' participation in the Population Commission appointed by the government in 1936 and shows how the Commission's policy recommendations conformed to their views. Concluding that the Myrdals "gave decisive direction and shape to the developing Swedish welfare state" (p. 174), Carlson argues that they achieved such influence because of their skillful use of

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social-scientific research and, above all, social-scientific jargon.

For all the merits of his approach, Carlson ends up exaggerating the influence of the Myrdals. Certainly, their view that the government should socialize the economic costs of parenthood prevailed from the late 1930s onward; but as Carlson himself notes, the Myrdals argued strongly for in-kind benefits (clothing, foodstuffs, housing, etc.) to ensure that state aid would improve the conditions of children—and on this score, their success was at best partial. The introduction of family allowances and the other major reforms of the 1940s represented a victory for the cash benefits line advocated by Gustav Möller, the minister of social affairs, in opposition to the paternalism that the Myrdals shared with the conservative approach to welfare policy (see Bo Rothstein, "Managing the Welfare State," *Scandinavian Political Studies*, 1987). As for women's employment and public day care, they did not become prominent features of the Swedish welfare state until the late 1960s. Family- and gender-related policy initiatives in the 1960s and 1970s might perhaps be seen as the final realization of the program that the Myrdals formulated in 1933–39, as Carlson suggests (pp. 200–202), but this delay suggests that Carlson's assessment of their influence must be qualified by taking into account the rise of the women's movement and the demographic and economic conditions of the new reformism. In other words, the role of intellectuals and ideas in the development of the welfare state must be situated within the broader explanatory framework set out by Olsson.

While Tilton and Carlson largely ignore the institutional structures and administrative politics of the welfare state, Olsson introduces this theme through his discussion of recent government efforts to render the public provision of welfare more efficient and more responsive to "consumer demands." Olsson does not articulate very clearly how he conceives the causal significance of institutional structures, however. Also, his treatment of recent developments does not adequately bring out conflicts of interest between public sector employees and their unions on the one hand and taxpayers, welfare consumers, and private sector unions on the other. These conflicts lie at the core of the current difficulties of Swedish social democracy.

Cornell University JONAS PONTUSSON

Insurgency and Terrorism: Inside Modern Revolutionary Warfare. By Bard E. O'Neill. McLean, VA: Brassey's, 1990. 171p. $19.00.

Attempts to devise analytical models for the general study of insurgency have invariably proved unsatisfactory, chiefly because it is such a multifaceted phenomenon, the combination of whose elements can vary greatly from case to case. A rather more modest enterprise has been undertaken by Bard O'Neill, namely, that of offering a framework setting out the main areas of analysis that must be included in any reasonably comprehensive study of insurgency. This framework is useful both for the individual case study and for comparative analysis.

So much of a partisan nature has been written about this subject that one must first commend the author for his admirable efforts at objectivity. Apart from one reference to "guerrilla-infested zones" (p. 130), O'Neill's approach is dispassionate and scholarly; he avoids the temptation to sensationalize his subject by unduly inflating the role of terrorism in insurgencies and he does not over-liberally attach the label terrorist to insurgents.

After a superficial survey of post-1945 insurgencies (in which East–West rivalries are arguably attributed too much weight vis-à-vis North–South themes), O'Neill follows with a general analytical framework. Readers are presented with seven types of insurgency (anarchist, egalitarian, traditionalist, pluralist, secessionist, reformist, and preservationist) and then introduced to four basic insurgent strategies (conspiracy, protracted popular war, military focus, and urban warfare), the first three of which are based partly on the Russian, Chinese, and Cuban revolutionary experiences. The framework also directs analysis to the environment of insurgency (both physical and human), popular support, organization and unity, external support, and government responses.

This kind of exercise has been done before, at least for terrorism. However, in the most stimulating part of the book, O'Neill goes beyond a simple listing and discussion of factors and types, to discuss the *relationships* between the different factors. For example, he looks at how government responses to insurgency can involve attempts to change its physical environment, as in the case of Hassan II's erection of Saharan sand walls in Morocco's response to Polisario.

Clearly written and succinct, the book will be of considerable value in encouraging university students and government advisers to adopt a more comprehensive approach to the study of insurgency. For scholars already immersed in studies, however, the insights offered by the book will be more limited, and the unevenness of O'Neill's acquaintance with the countries he cites as examples more evident. He knows the Middle East but is extremely unreliable on Latin America. For example, it is alleged that both the Montoneros and the Tupamaros lacked extensive apparatuses (p. 96); yet their building of substantial urban clandestine apparatuses was, in fact, a cause of rapid collapse in each case. It is claimed that the military is of little use against urban guerrillas (p. 128), yet clearly the military, not the police, destroyed urban guerrillas in both Argentina and Uruguay. It would appear that O'Neill has relied occasionally upon case studies prepared by graduate students and government officials (p. 12) and thus perpetuates mistakes in work that has not been subjected to criticism from real area specialists.

There is a weakness, too, in O'Neill's typology of insurgency. In particular, his egalitarian type is too broad, since it fails to discriminate between socialist insurgencies against a domestic establishment and "national liberation" struggles directed principally against foreign opponents. Moreover, in a book lacking a bibliography, some important sources are completely ignored. Guevara's *Guerrilla Warfare* (1961) is cited but there are no references to his subsequent texts revising the *foco* theory. One must also question the judgement of an author who devotes as much space to the distinction between active and passive support for insurgency as he gives to discussing the significance of the political system as an environmental factor.

In places, the analysis is distinctly low-level, and some of the points are embarrassingly bland. For example, in relation to popular support for insurgency, O'Neill writes, "The larger the groups that feel relatively de-

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prived, the greater the possibilities for mobilizing mass support and sustaining a widespread insurgency" (p. 74); and we are informed that for insurgents "sanctuaries in adjacent states are preferable to those a considerable distance away" (p. 119).

Yet, as a teaching aid, the author's sustained efforts to be systematic and neutral in his approach to insurgency make this a worthwhile introductory text. Teachers may want to question some of the categories and definitions employed; but it is a strength of O'Neill's framework that it is sufficiently flexible to allow for revision. It will be widely used in universities and military colleges.

University of Warwick RICHARD GILLESPIE

The Social Origins of Democratic Collapse: The First Portuguese Republic in the Global Economy. By Kathleen C. Schwartzman. Lawrence: University of Kansas Press, 1989. 224p. $29.95.

In 1910 a coalition of industrialists, workers, peasants, and liberal politicians overthrew the Portuguese monarchy and established the (first) Portuguese Republic. Throughout the next 16 years no less than 45 cabinets steered the unsteady republic through a succession of crises and coups. Finally, in 1926 the military restored "order," proclaiming the Estado Novo in the name of the church and nation.

Kathleen Schwartzman's purpose in *The Social Origins of Democratic Collapse* is to offer a comprehensive historical explanation for the rise and decline of the Portuguese Republic and, more broadly, to investigate the constraints that the world system places on democracy in a semi–peripheral nation. At the outset, she rejects Howard Wiarda's thesis that the authoritarian corporatism characteristic of the Estado Nova is a more or less permanent feature of the Iberian peninsula. Portugal's democratic interlude, although short–lived, "cannot be treated as an empirical aberration that preceded the authoritarian regime. It has to be taken seriously" (p. 25). Schwartzman also rejects analyses, such as Juan Linz's, that are narrowly restricted to political factors: "The potential for psychological or institutional reductionism and tautological explanations rests . . . on the erroneous assumption that the . . . political arena is autonomous" (p. 9).

Schwartzman ambitiously seeks to bring together two sets of literature that have unfortunately tended to be isolated from each other: (1) literature on the socioeconomic and class bases of dictatorship and democracy and (2) world systems literature. Following Barrington Moore and Nicos Poulantzas, she focuses on relations between agrarian elites, the nascent industrial bourgeoisie, peasants, and industrial workers. Although not entirely ignoring political factors, she emphasizes the structural preconditions for democracy. However, she further argues that Portugal's class structure can only be understood within the context of the world system. Of particular relevance here is Portugal's unique semiperipheral status within the early twentieth-century international division of labor. At this time Portugal was highly dependent on the core states (in important respects itself virtually a colony of Great Britain) yet simultaneously presided over a sizable empire.

The centerpiece of Schwartzman's conceptual framework is Antonio Gramsci's concept of a class project, namely, that stable parliamentary democracy requires the emergence of a hegemonic class faction (either industrialists or, perhaps, in the extreme periphery, rural gentry or landlords) capable of securing the acquiescence of subordinate classes to its own developmental project. Schwartzman reconstructs the various hypothetical possibilities for hegemonic class formation in early twentieth-century Portugal: agro-export, domestic agriculture, import-substituting industrialization, merchant capitalism. She argues that no fraction of the bourgeoisie was able successfully to forge such an alliance within the framework of parliamentary democracy.

The reasons for the inability to establish such an alliance can be found not only in the contradictions among the various segments of the bourgeoisie but also in Portugal's semiperipheral status within the world system. The crisis of global capitalism that culminated in World War I—especially the decline of British imperialism—precipitated a crisis for Portugal's traditional reliance on agro-export and merchant capitalism and its disarticulated economy. The crisis enhanced domestic centrifugal tendencies, thereby reducing the space for political compromise and rendering import-substituting industrialization (the logical next phase for Portugal) economically and politically unworkable. At the same time, the industrial working class was neither large enough nor radicalized enough to make its own revolution.

Ultimately, amid growing domestic and international chaos and the dissatisfaction of all classes and groups (including the trade unions) with the Republican politicians, the military stepped in to impose its own project. Unfortunately, while Schwartzman clearly demonstrates that the prevailing balance of class and international forces rendered a democratic project highly unlikely, she does not extend her analysis beyond the military takeover, although doing so might have provided additional evidence for her claims. What class interests, for example, were served in the Estado Novo?

The world system literature has often been criticized for not paying sufficient attention to national peculiarities. This carefully researched study effectively answers this criticism. While cultural and political factors undoubtedly contributed to democratic instability, Schwartzman's comprehensive analysis clearly shows the substantial relationship between the world political economy, Portugal's class structure, and the demise of the fledgling republic.

Hamilton College ALAN W. CAFRUNY

British Feminism in the Twentieth Century. Edited by Harold L. Smith. Amherst: University of Massachusetts Press, 1990. 214p. $27.50.

Journeys among Women: Feminism in Five Italian Cities. By Judith Adler Hellman. New York: Oxford University Press, 1987. 275p. $13.95 paper.

These two books undertake an examination of aspects of the women's movement that have received little attention from scholars in the field. In *British Feminism in the Twentieth Century*, Harold Smith has collected articles focused primarily on the activities of British feminists during the early 1900s. The contributors to this volume provide new insights into the feminism of the Pankhurst era and guide us through the movement's transformations into the 1950s. Judith Adler Hellman, in her exploration of contemporary feminism in Italian cities,

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makes the point that Italian feminists have developed programs that are either responses to, or reflections of, the particular traditions (especially of the Left) peculiar to their cities or regions. In both books the message is clear: we need to examine more fully the particular circumstances of feminist expression in order to understand the nature of feminist movements. Both books' authors underline the significance of time and space variables in the development of women's movement activities.

The Smith volume is, for the most part, a good collection of new work. Sandra Holton, in her examination of the romantic feminism of Emmeline Pankhurst, breathes new life into her discussion of the Pankhurst era by signaling the violent undertones of militant feminism and the potential for political assassinations that was part of the Pankhurst approach. The Pankhursts operated according to the principle that progress results from the actions of heroic individuals and cultivated heroines within their movement. But World War I intervened, and feminist violence was postponed, if not removed.

One form of violence that did emerge in the prewar years was the practice of wife beating. But as Jan Lambertz points out, sexual exploitation and cruelty to children proved to be much stronger rallying points for early British feminists, and the implications of unequal power relations between wives and husbands were not explored in early feminist analysis. This helps to explain the relative demise of feminism in the postwar era. The militancy at the turn of the century gave way to a moderate debate between proponents of equal rights and those arguing for women's special needs. By the 1930s, the force of British feminism had diminished and there was little progress for working women. Harold Smith's own contribution to this volume underlines the limits to legislative changes to improve women's status introduced by successive British governments in the post–World War I period.

The contributors are much less successful in explaining (or even describing) the development of feminism in Britain after the end of World War II. Contributors have explored women's role in the Labour party and Labour's feminist agenda, as well as images of women in the popular press and the work of Alva Myrdal and Viola Klein. But a stronger editorial hand could have integrated these now-scattered scholarly efforts into a more thorough (and needed) examination of the emergence of the contemporary women's movement in Great Britain.

Judith Adler Hellman's work provides us with a more thorough examination of the contemporary women's movement in Italy. Hellman opted for a research methodology that included participant observation of feminists in five Italian cities during 1977 and 1978. Whereas the Smith collection is rich in historical perspectives, Hellman's book opens our eyes to the culture-specific qualities of contemporary feminism and the intricacies of role interactions for Italian feminists.

Hellman explores the historical development and contemporary variations in Italian feminist thought and action, focusing specifically on the impact of leftist traditions in each of the five cities she examines. Her analysis is rich with descriptions of the special characteristics that distinguish the five cities in her study. For example,Turin is characterized by the traditions of a workers' city while Milan reflects the impact of student and New Left movements in the 1960s; and these two major centers, together with Rome, provided the models for feminist development in smaller centers throughout the country. Hellman's argument is that the issues developed and defined by feminists have varied according to location and that this has resulted in the emergence of a broadly defined social movement affecting women differently in different places.

But while Hellman refers in her conclusion to some general "laws of motion" common to social movements, her analysis would have been strengthened by a more consistent linkage of these common laws with her particular descriptions of feminist life in the five Italian cities. The reader is left wondering if the descriptions of differences may mask an equally significant pattern of similarities. Hellman argues that Italian feminism is unique, reflective of the leftist and Catholic subcultures that permeate Italian society. But she restricts her discussion to the Italian scene.

There are some problems with this volume. The author relies too heavily on acronyms throughout and should have provided a list of their references. Her interviews reflect attitudes in the late 1970s; but feminist thinking changed dramatically in the 1980s, particularly with the emergence of an antifeminist backlash. It is not clear why Rome, one of the cities Hellman signals as significant for the development of the movement, has been left out of this analysis. Finally, the development of the analysis for each of the five studies does not follow a common pattern and is thus somewhat hard to follow.

Nevertheless, this work is an important contribution to feminist literature and, considered together with the Harold Smith volume, suggests the need for more comparative analysis of feminist movements across, as well as within, countries. The Smith and Hellman works are not easily compared in their present format. However, the authors share some common perspectives and questions. These two books serve as timely reminders of the need for more rigorous cross-national comparative feminist research in the future.

University of Waterloo SANDRA BURT

Governing the Market: Economic Theory and the Role of Government in East Asian Industrialization. By Robert Wade. Princeton: Princeton University Press, 1990. 438p. $49.50.

Manufacturing Miracles: Paths of Industrialization in Latin America and East Asia. Edited by Gary Gereffi and Donald L. Wyman. Princeton: Princeton University Press, 1990. 416p. $49.50 cloth, $16.95 paper.

The newly industrializing East Asian countries are the focal point for two related debates in comparative political economy. The first concerns the role of state intervention in economic growth. Neoclassical accounts of East Asian growth do not neglect the role of the state; indeed, policy reform is at the forefront of those accounts. Yet the operative policies are the maintenance of macroeconomic stability; the provision of public goods; and trade, exchange rate, and pricing policies that allow firms to exploit comparative advantage. Sophisticated neoclassical approaches admit that the state may effectively intervene but only to allow relative prices of goods and factors to approximate those that would prevail in a situation of free trade. More targeted industrial policies that "distort" prices are seen as inimical to growth for two reasons. Not only do such policies reduce the efficiency of resource allocation, but they also encourage unproductive rent-seeking activity.

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Robert Wade's outstanding book on Taiwan builds on previous work by Chalmers Johnson to articulate an alternative *governed market* theory of East Asian success. In contrast to neoclassical accounts, the governed market approach emphasizes the role of government in encouraging both high *levels* of investment and a sectoral *composition* of investment that varies from what would have been supplied by the market.

Wade distinguishes between Industrial policies that "lead" the market and reflect government anticipation of future comparative advantage and policies that "follow" the market by supporting existing activities of firms. The case for the general market approach does not hinge solely on government leadership, since strong state support for firm-level initiatives may also contribute to high growth. Nonetheless, Wade pays particular attention to government initiative, arguing that "government policies deliberately got some prices 'wrong', so as to change the signals to which decentralized market agents responded, and also used non-price means to alter the behavior of market agents" (p. 29).

Unlike Alice Amsden's (1989) important study of Korea, (*Asia's Next Giant*), Wade's work is more cautious in giving the neoclassical account its due. An entire chapter is devoted to detailing the areas in which Taiwan's economic strategy matches the neoclassical account. Yet the core of the book (chaps. 4–6) provides a veritable catalog of state intervention In Taiwan. In addition to outlining the pervasive role of state-owned enterprises and the use of the financial and fiscal system to channel resources, Wade provides a detailed account of the trade regime, the core of the neoclassical account of export-led growth. Though exporters enjoyed a free trade regime for inputs, the domestic market for final products was protected in a variety of ways well into the 1980s and exporters were granted supports that went far beyond a favorable exchange rate.

The neoclassical rejoinder is that these policies did not matter or that they even slowed growth from what it might otherwise have been. Wade's response is to try to shift the onus of argument back onto neoclassical turf through the sheer weight of the empirical evidence in the form of case studies of sectors, careful attention to the timing of policy initiatives, and exhaustive reviews of the instruments of government policy.

Unfortunately, this strategy is unlikely to succeed. Greater attention must be paid to the theoretical foundation of the governed market approach, particularly the link between state intervention and growth. There are interesting such hints throughout Wade's account, including technological externalities, information costs, market failure, spreading risk, and dynamic comparative advantage. But none are consistently developed. The next step is clearly to build on some of the empirical insights provided by Wade, Amsden and others to provide the microeconomic foundations of the governed market approach.

The second theoretical debate surrounding the East Asian newly industrializing countries concerns why they pursued the development strategies they did. This is the central theme of the Gereffi and Wyman collection and is a leitmotif in the Wade volume, as well. This second debate cannot be separated from the question of *what* economic strategies the newly industrializing East Asian countries pursued. Gereffi's introduction and Colin Bradford's contribution both emphasize that the common distinction between Asia's export-led growth and Latin America's import-substituting industrialization should not be overdrawn. Nonetheless, the difference between the two regions in regard to broad industrial policy is the main puzzle the book seeks to address.

The concluding essay in the Gereffi and Wyman volume spells out the explanatory options in a "nested hierarchy" that includes 17 clusters of variables, from "world-system dynamics" to culture and ideologies. The result is a list that fails to discriminate among competing explanations. The more successful essays take some particular cut at the issue. The most theoretically succinct contribution is provided by Gustav Ranis, who argues that the absence of natural resource rents in East Asia made the continuation of import-substituting industrialization infeasible, while the larger Latin American countries could finance its continuation by taxing mineral and agricultural exports.

Three essays get substantial mileage from comparative analysis of differences in industrial structure across countries. Stallings emphasizes the effects of government policy on the structure, conduct, and thus perceived costs of multinational corporations. Gereffi and Fred Deyo use differences in industrial structure as a starting point for comparative discussions of the political role of big business and the popular sector, respectively.

Robert Kaufman and Tun-jen Cheng provide the most nuanced political analyses in the volume; interestingly, both attempt to underline differences within their respective regions. Analyzing the historical experiences of six Latin American countries, Kaufman traces the economic policy choices of the immediate post–World War II period to differences in political responses to the Great Depression. Cheng spins out the economic policy implications of single-party rule in Taiwan and bureaucratic authoritarianism in Korea, emphasizing the more interventionary policy style associated with the latter style of rule.

Robert Wade also places strong emphasis on the political foundations of Taiwan's economic development. He rightly notes the significance of authoritarian political structures and cohesive, insulated decision-making structures but mistakenly chooses to characterize the political system as "corporatist." This characterization fits Taiwan poorly and is, in fact, contradicted by the thrust of Wade's account. Wade concludes that Taiwan differs from Japan precisely in the weakly developed political linkages between the state and the private sector—an interesting puzzle for a model that stresses the "governance" of the market.

Wade's analysis of the policy regime is more developed than his analysis of policy choice. He appears reluctant to provide any theory of policy. Yet there are numerous interesting hints here, particularly about how the strength of the party and the relative weakness of business allowed for a policy equilibrium that provided economic opportunities for business but limited the chances either for rent-seeking or for speculative economic activities.

Wade has written an important book. It Is the best book to date on any of the newly industrializing East Asian countries and should be read by anyone interested in the comparative politics of growth. The Gereffi and Wyman collection is necessarily less focused but provides a solid example of the gains to be had from cross–regional comparisons.

Harvard University STEPHAN HAGGARD

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INTERNATIONAL RELATIONS

Eunomia: New Order for a New World. By Philip Allott. New York: Oxford University Press, 1990. 427pp. $27.95.

Philip Allott is ambitious in his goal for *Eunomia* (Greek, "good self-ordering of society"), hoping that it will "find a place in the tradition of the most general works of social theory of the past" (p. xxii). The book deserves to be taken seriously by intellectuals of many bents, for it cannot easily be pegged as a work of international politics nor even of political philosophy in any ordinary sense. It may influence generations of intellectuals who must grapple with our increasingly crowded playing field of life-threatening issues that are both the product of, and exacerbated by, humanity's outmoded political order.

The author was for many years a British diplomat; and his wide-ranging and imaginative interpretation of what's wrong with what we are forced to call international politics (forced, as the author reminds us, because of the poverty of the language that has emerged from our experience) was inspired by his conclusion that "British diplomacy had for centuries played a leading part in making a world-system whose peculiar rationality could also be seen as a form of madness" (p. xii). It is Allott's intent to make the reader understand the full extent of the madness and to demonstrate that to understand it thoroughly is to become empowered to overcome it.

Yet to put it that way in a sentence is to reduce the richness of the learning Allott brings to his creative work—from linguistics, philosophy, psychology, sociology, and law. This work continually articulates in the most startling and original ways what many of us may recognize as obvious when we see it stated thus. Then we note that the obvious is generally clouded except for the inchoate flashes of insight that we who work as scholars in this field may sometimes have into the truly crazy way in which the people of the earth have organized themselves for their global interactions. We usually suppress those insights, supposing them to be irrelevant—or, worse, unrealistic—flights of fancy distracting us from analysis of a still-brutish state of nature largely unredeemable at human hands.

Not so Philip Allott, who has spent 15 years painstakingly developing the ideas (and, one suspects, polishing the gleaming, frequently poetic prose) that have gone into this treatise. He had concluded during his time at the Foreign Office "that the old international order was neither a natural phenomenon to which humanity had simply adjusted its behavior nor a fortuitous aggregation of countless past events of human interaction. . . . The international system itself is nothing other than a structure of ideas; and it has been made nowhere else than in the human mind. The international order forms the minds of those who make the international order. The masters of the world of tomorrow are the slaves of yesterday's ideas" (pp. xv–xvi).

This conception of the problem has led him to begin his analysis with words themselves as they relate to thought about society, to reason, to human will, to power, to the search for order, and more. Then follows analysis of the constitution of society, only toward the end of which Allott begins to consider modern international society. The last section treats human well-being in the context of international order, concluding by depicting what humanity might become in the near future. The approach is frequently dialectical; the work is complex, original, protean, ironic. It is an example in the world of scholarship of the "preposterous idealism" Vaclav Havel would bring to political life (not to demean as other worldly and irrelevant to the "real" world of politics a work organically rooted in a solid understanding of humanity's psychological and social reality).

Familiar terms to the international relationist (e.g., *sovereignty, international organization, international law, deterrence*) take on new and multifaceted meanings as their place in the enslavement of both our minds and our ability to determine our planetary future is revealed. The end result is not a policy or a formula but merely a revelation of some of what we are capable of becoming.

On our atrophied social development beyond the state, Allott writes: "The self-conceiving of international society in the modern period . . . is a strange, not to say a tragic, story . . . which can be told in a single sentence. Tempted for a while to conceive of itself as a society, international society chose instead to regard itself as the state externalized, undemocratized, and unsocialized" (p. 240). On the nuclear threat, he writes: "A society which conceives its survival as depending on threats of self-destruction is a society which misconceives its nature as a society. A society which conceives its own self-destruction among the possibilities which it might choose is a society which misconceives its potentiality as a society" (pp. 252–53). These do no more than hint at the richness of the analysis. The book's full impact can only come by working one's way organically, as Allott has done, from "the word" in the beginning right up to "well-being" at the end.

Temple University LYNN H. MILLER

Elements of International Political Theory. By Michael Donelan. New York: Oxford Unviersity Press, 1990. 212p. $59.00.

Readers of this book may at first be puzzled or even irritated by its idiosyncracies. Instead of an argument, the author, who has taught international relations at the London School of Economics for many years, offers a dialogue between representatives of five major traditions: natural law, realism, fideism, rationalism, and historicism. This dialogue draws on the writings of philosophers and statesmen representing each tradition. The writing is old-fashioned and literary. A book so contrary to the objective style of contemporary political science would seem to have little chance of attracting the attention of international relations specialists, at least in this country.

This would be unfortunate, for several reasons. Donelan's book carries on, in a masterful way, the style of British international theory taught by Martin Wight and Hedley Bull. With its emphasis on history, morality, and

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philosophy, this kind of scholarship provides an alternative to the policy-oriented empiricism of U.S. social science. The latter has its virtues; but a sense of history, moral subtlety, and philosophical depth are not among them. Donelan has thought long and hard about his subject and can offer unexpected and genuinely illuminating insights into it. For example, he includes as one of the five major traditions fideism, a moral theology that divides the world into good and evil and articulates foreign policies of conquest or conversion based on this premise. The crusading spirit of the fideist is quite distinct from the compassion characteristic of natural law, or from the realist's prudent egoism.

Equally interesting is the author's extended exploration of historicism, another perspective that seldom appears in typologies of international thought. Against the dogmatic foundationalism of the other schools, the historicist insists on the particularity of moral and political experience. The historicist rejects the realist's mechanistic conception of politics, the natural lawyer's essentially static view of human nature and society, and the theoretical abstractions of the rationalist. Because what is happening now cannot be understood without knowing what happened before, real knowledge of international relations is historical knowledge. For the historicist, human affairs are best understood as a story.

Donelan's discussion of historicism usefully clarifies a confusion in current debates about "communitarian" theories of politics and international relations. We must, he argues, distinguish between the nationalist, who naviely thinks his own culture best, and the theorist of nationalism, who celebrates the idea of the national state rather than the actual culture of a particular nation. While the nationalist yearns for the independence of his nation, the theorist advocates national self-determination as a general principle of world order. But this ideology is itself a kind of nationalism: a projection on to the world stage of the values of a liberal culture.

As these sketches of fideism and historicism suggest, there are more voices in the great debate on international relations than those implied by the familiar categories *realism* and *idealism*. An enterprise like Donelan's raises four obvious questions, however. First, how should a perspective or tradition be defined? Is it, as the historicist might argue, a complicated and continuing story that cannot really be captured in the categories of a typology? Or is it an intellectual construct given boundaries by its own internal logic? Donelan seems not to have made up his mind on this question, which is indeed a hard one. Second, which are the really important traditions, and how many are there? Without criteria of importance and difference, these questions are unanswerable. Practically speaking, however, Donelan's fivefold typology is more useful than the realist/idealist dichotomy or the dominant British trilogy (realism, rationalism, and revolution) invented by Martin Wight.

Third, how skillfully is each tradition portrayed? Readers will inevitably disagree on the hits and misses here. I found the portraits of fideism and historicism more illuminating and exciting than those of realism and natural law, but this may say more about my own understanding than about the author's. I have the most doubts about rationalism, which, as Donelan constructs it, seems less an independent view than a composite of elements drawn from realism (the premise that mankind is permanently divided into separate states), natural law (that the relations among these states are governed by rules), and historicism (that these rules are based on the customs and agreements of the states themselves).

Finally, the reader will want to know which perspective is best or, at least, where the author himself stands. Donelan identifies himself as a rationalist, but he also provides a genuine dialogue that invites the reader to judge who has made the best case. It is precisely this refusal to push his own position that draws one into the conversation as a participant, not just an observer.

University of Wisconsin, Milwaukee TERRY NARDIN

The Power of Legitimacy among Nations. By Thomas M. Franck. New York: Oxford University Press, 1990. 303p. $32.50.

In the international system, rules are generally not enforced; yet they are mostly obeyed. International law, despite being primitive and decentralized, still generates habitual compliance with a vast array of sophisticated international legal rules, so much so that compliance often passes unnoticed and taken for granted. But why do powerful states obey powerless rules? What explains this incentive for states to comply with international precepts and standards? For Thomas Franck of the New York University School of Law, the answer resides in the power of legitimacy.

According to Franck, legitimacy embraces that quality of a rule or rulemaking institution that exerts normative influence toward compliance with its intended audience, largely because it functions in accord with generally accepted principles of "right process." To be sure, governments of states are willful creatures, capable of formulating and coexisting in accordance with community norms and standards. But the law governing the international community must express the general will of those governments, and here is where legitimacy comes into play.

Franck's argument is premised on the thesis that international law is not "law" in the same sense that it applies domestically. Though different from its municipal counterpart, the law of nations still retains great significance. Franck views international law as somewhat analogous to the house rules of a club, and it is the desire to be an accepted member of that club that motivates conformist behavior, i.e., compliance. Franck seeks to determine the extent to which state behavior can be explained by reference to rules and principles that undergird them. To accomplish this, he sets out four indicators, or properties, of legitimacy of the rulemaking process in the international community: determinacy, symbolic validation, coherence, and adherence.

Determinacy makes a rule's message clear. Unambiguous understanding adds impetus to rule-conforming behavior and fosters reciprocity. Legitimacy's cultural and anthropological dimensions are defined by notions of *symbolic validation*, complemented by *ritual* and *pedigree*. Here, communication is key, with emphasis on authenticity of a rule or rulemakers.

Interestingly, Franck excludes *justice* from the ingredients making for legitimacy. He does so on two grounds: (1) justice applies more aptly to persons than to artificial polities like states; (2) justice remains conditioned on a moral foundation, which makes it all too susceptible to

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convenient misinterpretation or subjective mishandling by the secular state.

Franck's argument is an engaging and compelling one. In its fundamental sense, a rule's legitimacy depends on its ability to exert compliance from states. In a voluntarist international community, such compliance pull is often evidenced by states' agreement to act in accordance with that rule. Obligation thus remains rooted in the concept of community. Put tersely, public support by the community of national governments remains essential to the effective functioning of international law.

Similarly, law furnishes a coercive but more cohesive order. The expectation of continuity induces compliance by generating a sense of obligation; or, as Franck puts it, "Legitimacy is the standard by which the community measures rules' capacity to obligate" (p. 206). Importantly, however, Franck is careful how he uses the word *law* in his analysis; he does not aim to suggest notions of a hierarchial international normative order. Rather, he seeks instead to portray actions of states within a community conception of obligation.

These insights represent important new thinking in light of the post–Cold War world. Franck rejects the prevalent view that state behavior is based wholly on self-interest, as narrowly conceived in power relationships. His argument is for a more complex appreciation of international relations—one that takes into account the commitment to rule-based principles and explains the willingness of states to cooperate, even when cooperation apparently might not serve national self-interests.

This is a lucid, profound study that deserves close reading by students and scholars alike. The author's points are well substantiated by historical examples and abundant authoritative citations. There is much to be gleaned about both the theoretical bedrock of international law and the intellectual construct of obligation that functions to persuade governments to comply with international rules.

A theory of legitimacy must probe ideas that are implicit, unacknowledged, and reflective of community will, as well as ideas that are explicit, articulated, and well recognized. This work laudably achieves that ambition. Indeed, viewed from the vantage point of the United Nations' forceful reaction to Iraq's brutal aggression against, and annexation of, Kuwait, Franck's study takes on even added importance. In short, Franck has given us a serious philosophical inquiry with a relevant message that makes eminent sense for contemporary international relations.

George Washington University CHRISTOPHER C. JOYNER

Cautious Revolution: The European Community Arrives. By Clifford Hackett. New York: Praeger, 1990. 239p. $42.95 cloth, $15.95 paper.

The Dynamics of European Integration. Edited by William Wallace. New York: Pinter, 1990. 308p. $54.00 cloth.

These volumes are timely and complementary analyses of various aspects of renewed European economic and political integration—the most immediately significant element therein being the creation of a single European market under European Community auspices by the end of 1992. Hackett's book is intended as a "primer" (p. vi) for those seeking to explore the history, nature, problems, and prospects of the European Community. The nature of his discussion, however, is sufficiently developed that even those familiar with the EC will benefit from his analysis. The Wallace volume, on the other hand, is intended for social scientists and others seeking a more detailed discussion of theories and patterns of integration that include, but also go beyond, the EC Integration is defined by Wallace as the "creation and maintenance of intense and diversified patterns of interaction among previously autonomous units" (p. 9). Where Hackett places his discussion of the EC in the wider global arena, including the implications for United States interests, the 18 contributors to the Wallace book (primarily European academics and policymakers) are more concerned about dissecting the formal and informal dimensions of European regional integration.

Hackett, executive director of the American Council for Jean Monnet Studies, sees the evolution of the EC as "one of the momentous events of the entire twentieth century" (p. 14) but an event that the U.S. government and academic community have tended to pay insufficient attention to. This book is thus intended as something of a corrective. While generally optimistic about the prospects for European integration under the EC, Hackett retains analytical balance. He does not, for example, dismiss emergence of a Fortress Europe, noting the possibility that "some member states, and some forces within them, will see a barrier-free (EC) market as an improved opportunity to exclude outside competition" (p. 100).

Nor does Hackett exclude the possibility that the ambitious goals of 1992 could be slowed by two major problems facing the 12 EC nations: an agricultural subsidy program (the Common Agricultural Policy) that eats up two-thirds of the EC budget and a significant lag in European high-technology capabilities as compared to the United States and Japan. On balance, however, the author tends to foresee progress toward economic integration, including the emergence of a European Central Bank and a single European currency, with a slower but significant evolution toward EC-wide foreign policy coordination.

Wallace, deputy director of the Royal Institute of International Affairs, is also somewhat critical of the academic community. In his perspective, however, the problem is that theories of integration (which flourished in the 1950s and 1960s with the work of such scholars as Ernst Haas and Karl Deutsch) have not been updated sufficiently to take into account new patterns of European economic and political integration. Thus, his edited volume seeks, among other things, "to begin to rebuild the conceptual debate on European integration" (p. 4). Two political scientists, Robert O. Keohane and Stanley Hoffman, however, criticize the bulk of the other contributions as having failed to deal "with major conceptual and theoretical issues, such as those raised by the integration literature of the 1950s and 1960s" (p. 276).

Keohane and Hoffman have a point: the Wallace book is heavily weighted toward an exploration of economic, technological, trade, and institutional patterns of European integration—seen as a system with a core and a periphery of states. The contributors are also interested in informal indicators of integration (e.g., cross-border population movements). The volume is a rich source of significant and not easily obtained data. While Wallace's

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aim to regenerate a discussion of theories of integration does not quite materialize, the book's other objectives are met, Keohane and Hoffman's well-taken point notwithstanding. It should be added, however, that their own attempt to unravel the relevance of earlier theories of integration is not wholly satisfying. For example, they assert that "perhaps a new theory of joint European decision-making should be invented, discarding loaded terms such as "supranationality" and "spillover," and drawing instead on contemporary theories of strategic choice in collective situations, or recent attempts to understand institutional innovation" and then proceed to relegate commentary on this to a note and to explore "some of the insights of neofunctionalist theories of change" (p. 284).

Political science increasingly needs to be part of multidisciplinary explorations of national and international political–economic developments. These volumes provide both specialists and nonspecialists with lucid—at times provocative—analyses. Both are calling upon students and specialists to reintegrate the study of European integration into the mainstream of social science concerns. They deserve an audience.

University of St. Thomas ROBERT L. FARLOW

Great Britain and the United States: Special Relations since World War II. By Robert M. Hathaway. Boston: Twayne, 1990. 172p. $25.95.

Losing an Empire, Finding a Role: An Introduction to British Foreign Policy since 1945. By David Sanders. New York: St. Martin's, 1989. 349p. $45.00.

Signals of War: The Falklands Conflict of 1982. By Lawrence Freedman and Virginia Gamba-Stonehouse. Princeton: Princeton University Press, 1991. 476p. $49.00 cloth. $14.95 paper.

In the post–World War II era, British foreign policy has been shaped by two long-term trends. First, Britain's relative economic strength has continued to decline. Second, the structure of Britain's economic interests has so shifted that Europe is now the centerpiece of the British trading network. In combination, these two trends have contributed to a fundamental restructuring of British foreign policy over the past 45 years. This process has led to three significant results: the abandonment of virtually all security-related commitments in the periphery, the gradual Europeanization of British economic and defense policies, and the steady deterioration of the special relationship Britain once enjoyed with the United States.

David Sanders and Robert M. Hathaway have both written informative, insightful, and highly readable historical accounts of this entire period of fundamental change and its consequences. Their points of emphasis are different. Sanders examines the evolution of British foreign policy vis-à-vis the empire, Europe, and the United States and argues that the changing pattern of Britain's external trade has been the decisive factor shaping the evolution of British foreign policy. Hathaway confines his analysis largely to Anglo–U.S. ties and the evolution of the special relationship and suggests that changing perceptions of the Soviet security threat and the steady deterioration of Britain's relative military capabilities have been the keys. Ultimately, however, both authors reach the same fundamental conclusion, namely, that due to structural changes in the nature of British power and interests, the future of British foreign policy lies in the direction of closer ties with Europe and a more detached stance in terms of both the periphery and the United States.

Both books also implicitly wrestle with an issue that international relations theorists are presently calling the agent–structure problem. At its core, the controversy centers around the relative importance of decision makers when it comes to accounting for international and foreign policy outcomes. The evidence they assemble tends to be impressionistic and largely anecdotal in nature. Sanders seeks to examine the worldview of British decision makers in an attempt to determine whether "realpolitik logic" has shaped policy deliberations and ultimately concludes that worldviews are basically inaccessible empirically. Hathaway focuses upon the interpersonal relationships enjoyed by successive British prime ministers and U.S. presidents and, similarly, fails to find compelling evidence that they matter a great deal in terms of shaping policy outcomes. The one possible exception to this generalization occurred in 1962 when President Kennedy authorized the sale of Polaris missiles to Britain "in large measure because of Kennedy's personal commitment to British–American ties and his fondness for Macmillian" (p. 65). Reluctantly, both Sanders and Hathaway seem to agree that when it comes to the agent–structure debate, neorealists have the better of the argument.

Whereas Sanders and Hathaway focus on the broad sweep of British foreign policy in the postwar era, Lawrence Freedman and Virginia Gamba-Stonehouse limit their attention to a single bloody episode: the Falklands conflict of January–June 1982. The result is a remarkably detailed case study that will be of interest to a wider audience than simply specialists in British and Argentine foreign policy. Students of decision making, coercive diplomacy, crisis management, international mediation, deterrence, and war all will find something of value. In short, this work is a major contribution to the literature.

The authors have produced a carefully crafted historical account based upon the analysis of recently declassified Argentine documents, personal interviews with unnamed British and Argentine officials, and existing scholarly research. The great strength of this work lies in the access it provides to the policy deliberations that went on in Argentina, Britain, the United States, and elsewhere. Numerous verbatim accounts of various policy debates are given. This technique lends a genuine sense of drama to the text, especially as we read some of the more riveting exchanges. Scholars interested in civilian–military relations will find the reported telephone conversation between Argentine president Galtieri and the commander of Argentine forces in the Falklands in the final hours of the war particularly memorable. Unfortunately, the sources of these accounts are not always identified by the authors. This raises some obvious questions about the reliability and authenticity of the reconstructions. This flaw, however, is not fatal and does not detract from the broader purpose of the study.

Freedman and Gamba-Stonehouse do not attempt to derive a set of lessons from this conflict. The narrative, however, speaks eloquently to the impact that uncertainty, misperception, bureaucratic, and domestic politics, as well as the sheer unpredictability of events, can

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have in shaping state policy and strategic interactions. This said, however, it is interesting to note that authors do not regard the Argentine decision to resort to force as a consequence of misperception. The Argentine government acted very much like a "rational actor"—carefully analyzing British interests in the Falklands, assessing the local military balance in the South Atlantic, speculating about possible international reactions to the invasion, and discussing the impact that occupying the islands likely would have on Britain's willingness to negotiate the transfer of sovereignty. The authors argue that based upon the evidence available at the time, the Argentine leadership reached a reasonable conclusion about the potential political utility of using force. The authors also demonstrate that there was nothing inevitable about Britain's decision to try and reclaim the islands. Indeed, a consensus on this point did not emerge within the British Cabinet until after the invasion actually took place.

Misperception did, however, figure prominently in shaping the final outcome of the conflict. During the last week in March the Argentine leadership became convinced that Britain had begun mobilizing its naval forces with the intent of reinforcing its position in the South Atlantic. In fact, however, no such decision had yet been made. On the basis of this misperception, however, Argentine leaders felt it was necessary to begin the invasion as soon as possible. Hence the military operation, which had been slated to begin on 15 May, was moved ahead a full six weeks. This was perhaps the most critical decision of the entire conflict. The authors argue that if Argentina had adhered to the original date, "the [planned] dispersal of the [British] fleet and the onset of the South Atlantic winter would have made . . . a response almost impossible" (p. 98). Ultimately, faulty intelligence, misread signals, and plain old dumb luck conspired to set the tempo of the engagement.

This point leads to a final observation. Like both Sanders and Hathaway, Freedman and Gamba-Stonehouse implicitly draw our attention back to the agent–structure problem; and once again, structural factors seem to have the better of the argument. This can be evidenced in the "deadlock" structure of British–Argentine negotiations prior to the invasion, the structural uncertainty characterizing both sides' evaluations of the other's intentions, motivations, and military mobilizations, and finally, the structure of the military confrontation itself. Taken together, all three books raise serious questions about the extent to which decision makers are really capable of steering events. They also point to the need for more explicit considerations of the agent–structure problem.

Virginia Polytechnic Institute and State University K. EDWARD SPIEZIO

China's Participation in the IMF, the World Bank, and GATT: Toward a Global Economic Order. By Harold K. Jacobson and Michel Oksenberg. Ann Arbor: University of Michigan Press, 1990. 199p. $32.50.

China and International Law: The Boundary Disputes. By Byron N. Tzou. New York: Praeger, 1990. 158p. $42.95.

The Chinese leadership's decision, beginning in the early 1970s, to rejoin the international community after a partially self-imposed period of isolation was greeted with enthusiasm by most other nations. The two volumes under review deal with two different questions addressed by the People's Republic of China's new outward-looking policy: global financial institutions and international law.

China joined the International Monetary Fund and the World Bank in 1980; and in 1986 it formally requested that negotiations be opened on its full participation in the General Agreement on Tariffs and Trade. The acceptance of the international financial order that was implicit in these actions reversed the largely autarkic economic policy China had pursued since 1949. China had extensive economic relations with the Soviet Union and Eastern Europe in the 1950s and cultivated commercial ties with Western Europe and Japan in the early 1960s. However, in both these cases the purpose of the external involvement was to build an integrated and self-reliant national economy. Chinese officials viewed keystone international economic organizations (KIEOs) as tools of capitalist exploitation, while on the other side, many a Western government sought to apply an economic embargo against China.

When China announced its intention to seek membership in the hitherto-despised KIEOs, there were mixed reactions. On the one hand, given China's huge size and population, its participation would represent real progress toward a global economic order. On the other, there were misgivings about how China's membership would affect the functioning of the KIEOs. Jacobson and Oksenberg posit four possibilities: (1) China would advocate major changes in the KIEOs that would benefit developing countries; (2) it would press for smaller changes that would allow its financial practices to be applied within the organizations; (3) it would demand to be treated as a special case; or (4) it would basically work within the framework of the KIEOs. In general, they believe, the fourth possibility comes closest to describing China's actual behavior. While China has been treated sympathetically, they argue, it has not been pandered to. Many of the allowances made for China are similar to those adopted for other large, less developed members, such as Egypt and Indonesia. At the same time, membership in the KIEOs has necessitated familiarity with the accounting techniques used by those organizations; and the Chinese have made certain adjustments to their own techniques. Pressure to release data so that China's applications for loans can be properly evaluated has not only made the Chinese system more transparent to outside observers but also expanded the number of Chinese who have access to data and who can therefore comment knowledgeably on policy choices.

The authors' apparent assumption that China's entrance to the General Agreement on Tariffs and Trade would be smoothly and expeditiously accomplished seems excessively optimistic. One wishes they had been more specific about the obstacles that must be overcome, since these have an important bearing on an assessment of China's participation in the world financial community. It is also unfortunate that the authors tend to treat China as a unitary actor. We know that there is substantial concern within certain segments of the Chinese leadership about even the modest amount of foreign debt that China has incurred. Some discussion of this and of what, if anything, it may mean for China's

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attitude toward the international financial order would be helpful.

Byron Tzou's focus is how China has used international law to deal with its various boundary disputes. He finds that China has in general operated within the constraints of international law, choosing only two of the many methods available (negotiation and mediation) and rejecting other recourses such as inquiry, conciliation, arbitration, and judicial settlement. Partly, this is due to the traditional Chinese distaste for judicial proceedings, even when the grievance is legitimate; and partly it is because China was for many years not a member of bodies such as the United Nations and was therefore understandably reluctant to use the adjudication methods provided by these organizations.

Tzou, born on the mainland and educated in Taiwan, is scrupulously objective in his treatment of the various disputes. His book is a model of organizational clarity, with each dispute treated separately before general conclusions are drawn. However, contrary to the author's assertion (p. 56), there *is* a border dispute between China and Vietnam. While the disputed land frontier between the two countries involves very little territory, it has the potential for taking on greater importance in the context of generally poor relations between the two states. Furthermore, disputes over the ownership of the Paracel and Spratley Islands have caused major frictions between China and Vietnam. The need to counter Vietnam's claims to the latter has resulted in the Chinese military's increasing its allocation for naval modernization and in the army's garrisoning arid and rocky islands at substantial financial and human costs.

Although both of these books are written primarily for specialists, the prose is in both cases admirably crisp and clear. Each work represents an important contribution to our understanding of China's international behavior.

University of Miami, Coral Gables JUNE TEUFEL DREYER

Cross, Crescent, and Sword: The Justification and Limitation of War in Western and Islamic Tradition. Edited by James Turner Johnson and John Kelsay. New York: Greenwood, 1990. 236p. $42.95.

The provisions of the tradition of the just war have become familiar in recent years to readers of professional literature and even news magazine cover stories about the morality of this or that war or this or that tactic. For a resort to war to be considered justifiable, *jus ad bellum* criteria must be satisfied. The resort must be defensive, the goal proper, and other remedies tried first; only duly constituted authorities may resort to war and only if there is reasonable chance of success. In the conduct of war, the *jus in bello* requirements of proportionality and noncombatant immunity must be respected. These features of this dominant Western tradition are deftly explored in papers by James Turner Johnson, Jeffrey Stout, Stephen E. Lammers, and Robert L. Phillips. Only Stout offers an original argument, namely, that Aquinas placed protecting justice ahead of any presumption against violence or warfare.

But what about the Muslims? Do the 800 million people inspired not by Aquinas and Grotius but by the prophet Muhammed and his followers accept these constraints on warfare? Or have their philosophers and theologians developed constraint notions that parallel those of the Western just war tradition? To a Western reader, at least, the most useful papers in this volume (which grew out of a conference) are those by specialists on Islamic thought who wrestle with these questions.

Abdulaziz A. Sachedina explores the notion of jihad. Though *jihad* should be translated not as "holy war" but rather as "*internal struggle*" or "*spiritual effort*," Islamic thought does seem to license not only war in defense of Islam but also "offensive endeavor in connection with efforts to bring about the kind of world order that the Qur'an envisions" (p. 39). Charles Butterworth's examination of the writings of tenth-century Muslim philosopher Abu Nasr al-Farabi reveals a much more generous warrant for warfare than one would find in the principle authors of the Western tradition (pp. 83–84). And John Kelsay agrees that much Islamic thought licenses military operations "to extend the boundaries of Islamic territory" (p. 200).

Though writers in the Western tradition have struggled, with some success, to reconcile revolt against oppression with the criterion that allows only authorities to initiate warfare, both Tamara Sonn and Khaled Abou El Fadl argue that Islamic thought, generally more authoritarian, is much less tolerant of rebellion and irregular tactics.

The Western principle of noncombatant immunity has been violated massively in twentieth-century high-technology wars of societies versus societies. But the principle survives as an embattled ideal. Islamic thought, Kelsay says, is much less clear about which people are fair targets and which must not be directly targeted—with an apparently more lenient position on the killing of enemy (infidel) civilians and prisoners.

Kelsay also suggests that paralleling the Western just war tradition, "a specifically Islamic contribution to the rules governing the conduct of modern war is still very much in process" (p. 217). It surely says something about the evolving Islamic view on these matters that the governments of the three dozen countries where most of the planet's 800 million Muslims live have all agreed to comply with the major documents of international law that codify and universalize the just war tradition's constraints (the UN Charter, Geneva and genocide conventions, etc.). These documents and their acceptance by governments representing Islamic populations are scarcely mentioned in this anthology. But doesn't this twentieth-century official embrace trump any classical or modern Islamic views that diverge from universally accepted (though oft-violated) international norms?

Purdue University JAMES A. STEGENGA

The Declining Hegemon: The United States and European Defense, 1960–1990. By Joseph Lepgold. New York: Greenwood, 1990. 225p. $45.00.

Joseph Lepgold's study of U.S. NATO policy represents the latest contribution to a second wave of research into the dynamics of international hegemony. The first wave, beginning in the mid-1970s, explored the relationship between hegemony and cooperation in the world economy. *The Declining Hegemon* (like other second-wave works, including Paul Kennedy's *Rise and Decline of the Great Powers* [1988] and Aaron Friedberg's *Weary Titan* [1988]) shifts our attention from economics to security

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affairs while examining the foreign policy problems posed by decline.

Echoing Kennedy and Friedberg, Lepgold emphasizes that hegemonic commitments tend to stick on the way down, despite intensifying trade-offs as relative resource constraints tighten. The heart of the book is an examination of the U.S. nuclear and conventional commitments to European defense between 1960 and 1980. The external context of U.S. policymaking changed significantly during this period (Soviet military capabilities steadily expanded, and the relative economic might of Western Europe grew substantially), yet U.S. policy changed only modestly. United States officials clung to extended deterrence even after the Soviet Union achieved strategic parity, attempting to bolster its credibility by devising new schemes for fighting a controlled nuclear war. Policymakers showed more flexibility on the conventional side. Monetary strains, congressional pressure, and growing European economic resources all prompted U.S. leaders to campaign for more equitable conventional burden sharing within the alliance. Nevertheless, these efforts to bring about marginal adjustments in the U.S. commitment were often pursued reluctantly and met with limited success.

Lepgold argues for a multilayered approach to explaining this pattern of delayed adaptation to decline. Systemic-level theories are, taken alone, inadequate to account for policy responses. Aside from their imprecision, systemic theories offer contradictory predictions. Realism suggests that declining resources should lead to a corresponding reduction in external commitments. On the other hand, deterrence theory points toward the importance of preserving commitments, even at the risk of overextension. A hegemon must be concerned that displays of weakness will lead allies to question its resolve or credibility, thus prompting defections.

In practice, executive branch officials have been most concerned about preserving U.S. credibility while congressional critics have been more preoccupied with overextension and burden sharing. Lepgold argues that U.S. policymakers have exaggerated the requirements of deterrence and alliance unity, tending to overestimate the likelihood of bandwagoning behavior. Thus, we must examine psychological beliefs, as well as systemic constraints in order to understand policy preferences.

Lastly, the ability to implement policy preferences is constrained by state structure, as well as by the nature of the issue area. Operating within a weak state structure, U.S. policymakers must sometimes accommodate congressional resistance. This constraint varies across issue areas, however. The president has greater autonomy over nuclear weapons policy than over conventional forces. Issue area also affects the leverage a hegemon has over its allies. Extended nuclear deterrence constitutes a public good, constraining the ability of the United States to limit its commitment or press for greater allied burden sharing. Conventional forces can be more easily controlled, and bargaining over relative contributions is more feasible.

Although dense and turgidly written, Lepgold's book provides a useful study of U.S. NATO policy. His attempt to apply the concept of hegemonic decline to security policy is welcome and interesting. The author's explanatory framework is, however, cumbersome; and his review of the relevant theoretical literature, while sophisticated, is overly long and eclectic. A more elegant and parsimonious approach to theory construction, along with a clearer consideration of alternative hypotheses and explanations, might have yielded predictions easier to generalize and evaluate. While the NATO case generally supports Lepgold's thesis of delayed adaptation, he himself notes several drawbacks. At some points, the United States seriously considered significant alteration of its European commitments. During the mid-1960s, for example, when McNamara flirted with a policy of no-nuclear-first-use. Lepgold acknowledges that NATO fails to provide a tough test of his argument: as an area of prime strategic significance, one would expect policymakers to adjust commitments elsewhere before altering ties with Western Europe.

Finally, while Lepgold concludes with a lucid discussion of recent and future U.S. NATO policy, he makes little attempt to relate the book's thesis to post–Cold War U.S. foreign policy. One wonders, in fact, whether an argument designed to explain the U.S. responses to gradual change in the bipolar structure of previous decades holds much relevance in the dramatically altered international environment of the present. Despite these qualms, Lepgold's book stands as a thoughtful addition to the growing literature on hegemony and foreign policy adaptation.

Drake University DAVID SKIDMORE

The Ways of Power: Patterns and Meaning in World Politics. By George Liska. Cambridge, MA: Basil Blackwell, 1990. 511p. $39.95.

The Ways of Power is an ambitious and panoramic analysis of global politics. Although the author's vision is grounded in classical realism, it is a highly sophisticated and self-conscious variant focusing on historical change of international systems and the actors that constituted them. Power, of course, plays a central role in the analysis; but its psychological and spiritual features are as central as are measurable capabilities. The state, too, is an omnipresent (though vaguely defined) element in the framework. For the author, any effort to equate the state with society must have "consequences fatal to community" (p. 205). Conflict and war are regarded as natural—even necessary—to give history an appropriate nudge "as a matter of natural selection of viable actors and agendas" (p. 228).

Actors and systems are seen as inevitably and naturally evolving through distinct stages in recurring cycles. Just as actors evolve from city–states to larger entities and then mature and weaken, so do systems of states expand and later contract. The author illustrates these sequences in system–actor development by reference to numerous historical periods and regions from ancient Mesopotamia to the present; but the stages themselves (like other typologies the author elaborates) are asserted, rather than justified, and so seem arbitrary. There are many ways to sort the same information, and the reader may feel somewhat overwhelmed by the trichotomies and triads that are heaped up. Indeed, on occasion, it appears that history is forced to shape itself to Liska's typologies.

In Liska's historical framework, structure and changes in structure are viewed as largely deterministic; and structure (and therefore behavior) are regarded as the products of a time–space continuum—what Liska calls "political physics." This is not, however, the Newtonian

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physics of traditional and static realism but realism influenced by Einstein's relativity. Consequently, different actors and systems will simultaneously be at different points on the time–space continuum and will evolve at different rates of speed; and the behavior of actors will be largely determined by their position in systemic schisms along with their place on the rise–decline curve.

Like other realists, Liska does not regard history as equivalent to progress and believes it is folly for leaders to try to channel history in particular directions. But instead of simply denouncing "utopians," who seek to deny the realities of structure, Liska describes what he calls "politicized poetics." Global politics is repeatedly characterized by one of two types of tragedy. The first is a two-power conflict in which the contestants destroy one another "in casting out imagined or exaggerated evil" (p. 189). The second, which Liska calls "the tragic hero in interstate relations," is the actor who seeks hegemony in defiance of structural and normative impediments. Such an actor is doomed because it tries "to abrogate the laws of the relevant nature by escaping not only the chain of conflicts with equal powers but also and mainly the next turn in the cycle of rise and decline deputizing for birth and death among mortal states" (p. 190).

What is it that enforces the iron laws of history and so limits the potential for political genius? Unsurprisingly, it is that old realist standby, the balance of power. Liska's analysis of the balance is less original than his vision of a dynamic global structure; and that analysis leaves unresolved most of the logical and empirical issues associated with traditional balance-of-power theory. Nevertheless, he does admit that "perfect" equipoise is an ideal rarely met because of the dynamic nature of political systems.

The book first provides readers with the author's core concepts and typologies, then illustrates them with a galaxy of historical cases. Additional theoretical chapters follow; and the final section of the book applies the concepts and theories to a variety of issues in contemporary world politics. Although the book was completed before major events like the reunification of Germany and the war in the Persian Gulf, the analysis remains remarkably fresh. The Cold War, as the author interprets it, passed through the major cleavages characteristic of international systems (East vs. West, land vs. sea power); and the alternatives confronting the superpowers (e.g., a condominium of the two, the effort by one to create unipolarity, etc.) are, in Liska's view, those that great powers have faced many times before.

The Ways of Power is a genuinely original, timely, and important contribution to international relations theory. The influence and continued relevance of Aristotle, Thucydides, Machiavelli (theoretical chapters are labeled "discourses"), and other classics are repeatedly revealed in the analysis. Whatever one's view of the realist tradition, this volume is an authentic addition to it and extension of it. Ultimately, if the volume is not as widely read as it deserves, this will be largely a result of the author's unusually periphrastic style that obscures important points and renders virtually impenetrable whole paragraphs and pages.

Iowa State University RICHARD W. MANSBACH

Exporting Democracy: The United States and Latin America. Edited by Abraham F. Lowenthal. Baltimore: Johns Hopkins University Press, 1991. 422p. $55.00 cloth, $12.95 paper.

U.S. Policy in Central America: The Endless Debate. By Dario Moreno. Miami: Florida International University Press, 1990. 186p. $26.95 cloth, $14.95 paper.

The Ordeal of Hegemony: The United States and Latin America. By Guy Poitras. Boulder: Westview, 1990. 214p. $34.95.

These three books share some common underlying political assumptions largely shaped by the fevered and prolonged debate over Central America policy in the 1980s. Hence, they are already dated, somewhat overwrought, a bit passé, and quite one-sided in a number of their assumptions and analyses. In all three, scholarship too often yields to polemics and partisanship.

Dario Moreno's *U.S. Policy in Central America* originated as a doctoral dissertation at the University of Southern California. While containing some valuable analysis, its subtitle, *The Endless Debate*, shows how the book has been overtaken by events. Central America is now largely out of the headlines; the vigorous policy conflict of the 1980s is over; and thanks to Secretary of State James Baker's skillful political maneuvering, Central America and its problems have been largely finessed (albeit not "solved"). In addition, Moreno's analysis of the assumptions and processes of Reagan administration policy on Nicaragua and El Salvador is often partisan and misleading.

Guy Poitras's *Ordeal of Hegemony* has its origins in a well-known article (Lowenthal, Abraham F. "The United States and Latin America: Ending the Hegemonic Presumption" *Foreign Affairs*. 1976.55:199–213). This was a Carteresque article that served as a basis for some of the thinking of the new president's Latin America advisers. But that view, which implies a unilateral reduction of the U.S. role in the world (particularly in this hemisphere) has been repudiated overwhelmingly by the voters in the last three presidential elections. Moreover, with the decline of the Soviet Union and with Europe and Japan showing little interest, Latin America has been thrown back more strongly than ever into the arms of the United States, whether or not we (or they) wish it or not.

Abraham Lowenthal's edited *Exporting Democracy* is disappointing. Well funded and with several reputable contributors, it is nevertheless weaker and more dated than one would expect. First, the book has no statement of themes, introduction, or discussion or definition of what Latin America means by *democracy* (often closer to a Rousseauian than a Lockean conception). Second, while most of the authors in the book are committed Democrats, now that the democracy–human rights agenda has largely been taken over by Republican administrations, they are forced to be more critical of it. Third, the historical approach employed in the book emphasizes Latin America's often unhappy past experiences with democracy and the frequently mixed motives of U.S. foreign policy but contains no information on what is new in Latin America to provide a more solid basis for democracy (greater affluence, larger middle class, rising literacy, greater institutionalization, etc.); nor does it come to grips with the changes in U.S. policy that led us finally to conclude that democracy is the best basis for promoting U.S. security interests. We should

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further say that the selection of contributors produced a quite uneven book, with several mediocre chapters and the glaring omission of many of the leading writers on the subject.

Actually, once one gets past the pretentious, inflated rhetoric, and partisanship, there is much in these three volumes to recommend them. Moreno usefully analyzes the domestic debate over Central America policy in terms of the familiar division between "Cold War internationalist" (which he identifies with the Reagan administration) and the "post–Cold War internationalists" (which he identifies with the Carter administration and the present Democratic government-in-waiting). He argues that both the Carter and Reagan administrations faced failure in their policy because of lack of foreign policy consensus since Vietnam and the democratization of foreign policy, which made domestic forces sufficiently powerful to moderate and even hamstring national policy. He dissects the rhetoric of both these recent more ideological administrations and then in the substantive part of his book shows how domestic politics prevented both from carrying out their agendas. This is solid material, some of it fresh and original; and Moreno's rendering of it is generally balanced. However, he still sometimes falls into the trap of repeating the tired and inaccurate clichés that the Reagan administration was "obsessed" with Nicaragua and that it only sought "military solutions"; moreover, his sources are similarly heavily weighted toward one side of the political spectrum and from persons in a previous administration not usually thought of as the high point of U.S. foreign policy success.

Poitras's book is an ofttimes thoughtful overview of U.S. policy in Latin America. His theme is the presumed decline of U.S. hegemony in the Western hemisphere. He analyzes the history of U.S. policy in Latin America and goes through different scenarios of the "decline" literature. He looks at U.S. security policy, Central America, economics and the debt issue, and U.S. intervention. However, he also repeats some of the hackneyed phrases of the 1980s, such as that the United States is a "slowly fading giant" and that it is "irresistibly drawn to intervention." Poitras has clearly been caught up in the political–ideological quarrels over Latin America during the past decade. But he is a senior scholar, his earlier study on *Latin America: The Politics of Immobility* is a fine book, and he is too thoughtful to be just another ideologue. Thus, Poitras repeatedly states his decline-of-hegemony thesis and then backs away from it as his own analysis leads him to other conclusions. For example, Poitras sees international debt as a major factor in indicating U.S. decline but is forced to conclude that the debt has actually increased U.S. power in Latin America. He concludes the book not on the hegemony theme or the presumed decline thereof but on the familiar note of calling for greater cooperation and the finding of common ground between the United States and Latin America. One suspects that the reason for his ambiguity is that he began the book during the white heat of the divisive Central American debate but finished it when the policies of the 1980s had begun to bear positive fruit or been moderated: the Nicaragua election, the U.S.–Soviet approchement, a succession of disarmament treaties, greater global cooperation, and even an arguably more mature U.S. policy in Latin America.

Lowenthal's edited volume is plagued by many of the same problems. It contains four historical chapters: Paul Drake on the Wilson period, Leslie Bethell on the early Cold War, Tony Smith on the Alliance for Progress, and Thomas Carothers on the Reagan Era. While containing valuable information and analyses, the common thread that runs through these chapters is hostility to most aspects of U.S. policy. The country chapters (Argentina by Carlos Escudé, Chile by Heraldo Muñoz, the Dominican Republic by Jonathan Hartlyn, Mexico by Lorenzo Meyer, and Nicaragua by Joseph Tulchin and Knut Walter) also have much worthwhile data and insight; but several are marred by talk of numerous "contradictions" and by excessively critical, although usually unsubstantiated, comments about recent U.S. administrations. The most refreshingly original chapter is by Elizabeth Cobbs on U.S. business in Latin America where she points out that business policy has often been far ahead of, and more enlightened than, U.S. official policy. As well, Cobbs's may also be among the few studies dealing with a glaring omission in the field—the relations of business to political development. Lawrence Whitehead penultimately discusses the international role in assisting democracy; and the editor adds an uninspiring conclusion—a shame, because, given the importance of the topics and the quality of many of the contributions, this could have been an excellent and major study.

These three books and so many others lead one to worry about the field of Latin American studies. Because of the intense conflicts of the 1970s and 1980s, has the field now become so politicized as to all-but-rule-out serious, genuinely *objective* scholarship? Not quite, but we have come perilously close to that point.

University of Massachusetts HOWARD J. WIARDA

Inventing Accuracy: A Historical Sociology Of Nuclear Missile Guidance. By Donald MacKenzie. Cambridge: Massachusetts Institute of Technology Press. 1990. 464p. $29.95.

Inventing Accuracy recounts the development of nuclear missile guidance in the United States and the Soviet Union. It is based on available documents plus 140 interviews with guidance and navigation technologists, navy and air force officers, and defense officials.

The thick description of missile guidance technology provides information about an important element in the nuclear command-and-control process. It adds new detail to public knowledge of the area known as C^3I (Command, Control, Communications, and Intelligence) and makes it accessible to a wider audience. This knowledge is relevant background to understanding smart nonnuclear missiles in the post–Cold War era (e.g., the accuracy of the Patriot missile in the Persian Gulf).

At the same time, the book brings new concerns to weapons development case studies. Where previous work has focused on the high politics of weapons, this study shows that low politics is critically important as well. Since it provides technical details about the black box of inertial guidance, it also shows that this black box includes politics. The technical is not naturally isolated from the political—it is joined with it in a "seamless web" (p. 411). This web goes well beyond and below our received understandings of bureaucratic politics, into

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the sheltered laboratories of science itself, into the politics of the ordinary. The state and political decision are disaggregated to an infinite level of detail: "The black box of technology is actually a Russian doll. . . . Only the analyst's energy and the reader's patience provide a limit" (p. 393).

The micro details of technology also provide a view onto the macro workings of the international political system they support: "Missile guidance forms a technological 'window' into crucial divides in nuclear thinking," including both the technical and political aspects of the "projection of military power over long distances" (pp. 2, 385). The study shows that important aspects of the international system are not given but made, and it shows how they are made. It is thus an addition to general theory and research in international relations.

In addition to substance, *Inventing Accuracy* unobtrusively provides interesting methodological contrasts and insights. It takes the approach of historical sociology, using qualitative oral interviews and written documents as data. It is an ethnomethodological anthropology, "a journey among a tribe, which, to most of us is unknown and alien" (p. 25). Yet this tribe is quite mathematical, and one of its central concerns is "the theoretical understanding of errors" and the mathematical modeling of error processes (p. 349). Consequently, Mackenzie's study of missile accuracy treats the reader to a detailed discussion of the error term in the guidance equation, which can easily be extended by analogy into political science. The discussion of missile error budgets includes a standard discussion of differentiation between systematic bias and random error. Beyond this, however, there is an explanation of sophisticated techniques for the estimation of error component parameters, empirical extrapolation of error in different contexts, and error compensation algorithms.

Finally, *Inventing Accuracy* is a contribution to political epistemology. Mackenzie challenges both technological and political determinism. The central theme of the book is that science and technology are constructed, not inevitable. Science is "a historical product and social creation" (p. 2). There is no "hard fact," no "atomic fact" with "absolute warrant," only flexible "knowledge claims" (pp. 10, 381). Parts of the "network of knowledge" may be more or less "well-entrenched" (p. 364). Nevertheless, claims can never be conclusively and finally tested, proven true and valid once and for all. They are always open to challenge, even though such challenge may not actually be made (p. 10). Certainty tends to vary with the distance from knowledge production. Naive knowledge consumers tend to be true believers to a greater extent than sophisticated producers (p. 371). Indeed, the producers have an important interest in maintaining the hardness of fact and the autonomy of science precisely so that external confidence and support will continue. They are the basis of the expert's power and authority.

Science and technology take the form they do because of the choices, decisions, and behavior of actors living in a particular historical, social world. The past included different possible alternative political paths: "Different technical pathways are seen by those involved as having different military and political meanings" (p. 19). Science and technology could thus have been discovered and invented differently had the actors lived in different times or place, seen the world differently, or chosen or acted differently in the past.

More importantly, different alternative paths stretch equipossibly into the future. Aspects of contemporary science and technology can thus be remolded if new actors perceive and choose differently in the future. Scientists can imagine and construct different possible sciences. The political sphere is much wider than we have previously believed. Scientists, including both natural and social scientists, are political actors not only when they vote but also in their everyday scientific lives.

Modern weapons are not inevitable, either their creation or continued existence; and neither is the larger international political system that they support and in which they are embedded. Both the technological and political systems can be deconstructed, "uninvented" through continuation of the same process that created them (p. 424). The Pandora's Box of a missile-based international system cannot be closed at once. But perhaps it may gradually be taken apart, enclosed in another container, or simply left behind.

University of Colorado FRANCIS BEER

The Myth of America's Decline: Leading the World Economy into the 1990s. By Harry R. Nau. New York: Oxford University Press, 1990. 424p. $29.95.

Such is the plethora of books on U.S. hegemony (or decline thereof) that one's reaction to the title of Nau's book is an unenthusiastic groan. Yet the book does reward a good reading. It is not merely a rehash of tired academic arguments about hegemonic stability but a sensitive account of international economic diplomacy, emphasizing the Reagan years. A former National Security Council staffer, Nau's theme is the importance of ideas and policy choices, rather than exogenous structural factors.

The book covers the Bretton Woods conference up to the end of the 1980s. The descriptive theme is that although the coercive power of the United States has declined in relative terms, the United States has retained its ability to achieve desired outcomes by seeking "common political purposes and common economic policy standards" with other countries (p. 369). The world is, he believes, converging on a commonly held set of norms and values that guide economic policy; and this consensus has served to offset the decline of U.S. power. The descriptive theme underpins the explanatory argument. Structural explanations of outcomes (those relying on exogenous variables, such as power concentration, systemic polarity, etc.) fail to provide an adequate explanation because they neglect ideas and the policy choices that follow from the competition of ideas. The vicissitudes of U.S. foreign economic policy have been due as much to policy purpose and choices as to structural determinants.

Explanation leads to prescription; and Nau has some strong rules that he feels ought to guide international policymakers: "America has a choice to reaffirm Western values of human freedom and individual dignity and to reapply the efficient economic policy standards of price stability, flexible domestic markets, and internationally freer trade of the Bretton Woods era" (p. 370). Unfortunately, Nau does not seem sure of how his desired policies are to be achieved—what practical mechanisms will lead to the consensus he prescribes. His disparaging comments on bureaucratic politics and organizational

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design as policy remedies and his moderate praise of Reagan's economic vision suggest that Nau may hope that his prescription would appeal to a suitable political entrepreneur.

The organization of the book is simple. Part 1 sets the framework by contrasting the structural approach with Nau's "choice-oriented perspective."Purpose and policy, not power and politics, drive outcomes. The distinction may not be as clear as Nau believes (he confesses the possibility, p. 51) and is disadvantaged by the fact that his argument for the primacy of policy depends on his own interpretation of historically unique events. Some of his comments on alternative models are a little puzzling (e.g., the assertion that market-based models assume harmony of interests and an egalitarian distribution of power, pp. 66–67).

Parts 2 and 3 examine the rise and fall of the Bretton Woods system. The United States was slow to take advantage of its postwar opportunity to lead a consensus around Bretton Woods values, until the Marshall Plan provided the impetus. Nau follows the familiar story that the postwar economic system was built around the Western security system as a "shared political community." His account of the fall of Bretton Woods is more original. Contrary to those who have suggested that Bretton Woods fell because the lack of an adjustment mechanism led to the need for dollar-based liquidity, which in turn undermined confidence in the convertibility of dollars, Nau argues that there was no inherent structural instability in the system. Claiming that there was no liquidity problem, Nau states that the problem was policy failure in the United States in the form of an unwillingness to contemplate deflationary macroeconomic policies in the 1960s. The United States was, he says, "unwilling to recognize the roots of instability in domestic politics [and] blamed the international system" (p. 148). Hence, the deliberate smashing of the system in 1971 and a decade of policy drift. The Reagan administration faced, he believes, a unique opportunity to return to good policy—the "Bretton Woods triad"—but failed to do so for reasons that I, at least, never understood. Nau's explanation is that "the Reagan Administration lacked a full, integrated, intellectual understanding of its programs and failed to generate the political will and support to follow through on its programs" (p. 223). This might describe any government. The book returns again and again to this curious lacuna: we need good policy leadership, but Nau is reluctant to tell us how to get it.

University of Iowa JOHN A. C. CONYBEARE

The Soviet Union in the Horn of Africa. By Robert G. Patman. Cambridge: Cambridge University Press, 1990. 411p. $49.50.

Soviet Strategy in Southern Africa: Gorbachev's Pragmatic Approach. By Peter Vanneman. Stanford: Hoover Institution Press, 1990. 142p. $15.95.

The Soviet Union has a surprisingly long history of relations with the countries, conflicts, and political movements of Africa. Indeed, since the successful conclusion of the Bolshevik Revolution, the Soviets have—with varying degrees of commitment—promoted the cause of national liberation in their foreign policy. First under the aegis of the Communist International, later with the provision of military equipment and training to insurgent guerrilla groups in the 1950s and 1960s, and finally with the direct assistance of Soviet and Cuban military personnel in a variety of brush war conflicts in Africa during the 1970s and 1980s, the Soviet Union has championed the plight of revolutionary groups and fledgling socialist governments in black Africa.

There was a period of increased Soviet activism in Africa during the 1970s when the Soviet Union was involved in several regional disputes and revolutionary changes, principally in Southern Africa and around the Horn of Africa. During this period, Soviet optimism about the success of the Socialist model, the "leading role" of the vanguard party, and the power of the Soviet state was at its peak. The Soviet Union became primarily engaged in the endgame after the collapse of the Portuguese empire in Southern Africa and sponsored local Marxists to power in both Angola and Mozambique. Subsequently, the Soviet Union sided with Ethiopia against its erstwhile ally, Somalia, and again sent massive shipments of armaments to the region.

However, the optimism of the 1970s faded by the middle of the 1980s. These outposts of empire and the fledgling Soviet foothold in Africa were undermined by local and international forces that conspired to showcase the limits of Soviet influence. The Reagan Doctrine of championing anti-Soviet insurgencies in the Third World did much to tax Soviet energies and extend them in the peripheries; and the Soviet Union did little to alleviate the dire economic predicaments of its local allies. Even before the era of Gorbachev and perestroika, the Soviet Union had learned the traditional lessons of imperial powers with far-flung holdings: it is easier to undermine an empire than to hold it. Or as one Southern African politician translated it to an African context, "it is easier to be a poacher than to be a gameskeeper."

The Soviet involvement and intervention in African politics and areas far from its territory spawned a tremendous literature in the West concerning the sources, methods, and achievements of Soviet foreign policy. Much of this Western literature hoped to describe a complex set of motivations that led the Soviet Union to engage its weapons and prestige in far-flung corners of the globe. However, the current circumstances of the Soviet Union have changed so dramatically since the African activism of the 1970s that the study of Soviet–African relations has been rendered virtually obsolete. With the collapse of pro-Soviet regimes in Eastern Europe and the increasing signs of Soviet disorder and decline, Soviet policy toward Africa has received much less academic attention in the last several years. Yet the Soviet Union, perhaps contrary to conventional wisdom, has persevered in its political, military, and diplomatic initiatives in Africa long after there were any identifiable geopolitical benefits or regional rewards to be gained.

Trying to explain this persistence is the subject of these two very able, but very different, efforts on Soviet policy in Africa. The first, Robert G. Patman's *Soviet Union in the Horn of Africa*, traces the diplomacy of intervention and disengagement of the Soviet Union in the Horn of Africa from its earliest periods. Patman draws on extensive Soviet and Russian source material to examine the earliest Russian interest from the czarist period to the initial era of Soviet rule and subsequently through the Cold War in Africa to the current period of Gorbachev and retrenchment.

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Patman makes a persuasive case that Soviet activities in the Horn cannot be neatly compartmentalized into Africanist or globalist schools of thought. The Africanist approach, he explains, "emphasizes that Soviet policy in the region lacked coherence and was essentially reactive and opportunistic in the face of a local conflict that was not of Moscow's making." In contrast, he explains, the globalist school of thought argues that "Soviet behavior in the Horn was consistent with the grand design to undermine Western and Chinese influence wherever possible through the cynical manipulation of local grievances" (p. xiii). Patman's book suggests that there is a better explanation for Soviet involvements, which he describes as an approach of "calculated opportunism" that is based more on short-term openings and initiatives than on long-term strategic blueprints.

Patman provides us with a thorough accounting of a period of Soviet adventurism in the Horn that appears to be coming to a close. He has managed to sift through a tremendous range of Soviet sources to trace the Soviet initiatives during its most important period, 1973–79. Patman also offers lucid analysis of the expansion of Soviet–Somali ties in the late 1960s and early 1970s and the disputes in Soviet–Somali relations before the Ethiopian revolution in 1974. On several occasions he points to Western misreading of Soviet intentions in Ethiopia and makes a powerful case for the reasons and rationale that led to the Soviet intervention. Patman then takes us through the sometimes contentious relations between Mengistu and a procession of Soviet leaders and the subsequent decisions to reassess Soviet policies towards Ethiopia and the Horn at large. The book also provides a fascinating and detailed description of the Ogaden campaign and the international diplomacy and intervention that shaped the war.

The only major shortcoming in this generally informative book is that while there is a tremendous record of the intervention, there is not nearly enough discussion about the issues that have led to the Soviet disengagement and diplomatic efforts to promote national reconciliation and retreat. The shifts in Soviet foreign policy from confrontation to cooperation and even condominium vis-à-vis the United States and the international community needs much greater amplification. In this sense, there is little context for current Soviet policies in the Horn among the larger dilemmas of Soviet policy in the fall of Eastern Europe and the dire domestic problems facing the Soviet leadership at home. While Patman rightly states at the outset that Western analysts ignore Soviet writings at their peril, a minor shortcoming of his own book is that he has not consulted a number of important Western studies about change and continuity in Soviet foreign policy. It is, however, an important historical rendering of the complex history of conflict, discord, and disengagement that marks Soviet policy toward the Horn.

A second book explores very different territory, Peter Vanneman's *Soviet Strategy in Southern Africa*. Vanneman was one of the first Western commentators to explore Soviet policy in Southern Africa; and his work in the early 1980s (particularly on the Soviet involvement in the wars of Southern Africa), played an important role in helping to understand the scale and significance of foreign intervention in the region. His book is subtitled *Gorbachev's Pragmatic Approach* and makes a rather dubious case that Southern Africa still plays an important role in the formulation and execution of Soviet foreign policy. Vanneman cites the meetings between Soviet and African dignitaries, various international discussions about the new directions of Soviet policy in Southern Africa, and the growth of personnel in Soviet embassies in the region as evidence of increasing Soviet interest toward the region. He uses the same prism and measurements of Sovietology that were more relevant in the past—ideological imperatives, geostrategic motivations, and the extension of empire. Yet, this so-called Soviet strategy in Southern Africa has been ambushed by the historic changes in the Soviet polity, rendering this traditional approach of Western Sovietology virtually irrelevant.

Vanneman does describe a Soviet Union that has played a prominent military role in the past but is determined to share in a larger political and diplomatic role in the emerging new political realities in Southern Africa. Here, his overall analysis is essentially sound, but some of his claims have been overtaken by events. For instance, the perceived Soviet interest in basing rights or air facilities in Southern Africa can hardly be seen as important, given the magnitude of the Soviet military retreats in arms control negotiation and in its Eastern European presence in the last two years.

Both Vanneman and Patman make a powerful case that Soviet policy in Africa is out of phase with the larger challenges and directions of the Soviet Union's foreign and domestic policy. Although the Soviet Union has seen drastic setbacks in such central strategic areas of East–West contention as Eastern Europe, it persists in competing for influence on the peripheries. Indeed, it is almost as if the global game for influence has been declared over by political elites in Washington and Moscow but someone has forgotten to inform the local agents and embassies of this decision, so that the proxy wars, desperate diplomacy, and clandestine meetings continue in Soviet policy toward Africa.

Harvard University KURT M. CAMPBELL

Nuclear Deterrence Theory: The Search for Credibility. By Robert Powell. Cambridge: Cambridge University Press, 1990. 230p. $47.50.

History has not been kind to the concerns of Robert Powell's new book, which appear exceptionally untimely in the current climate of superpower relations. Its technical style, too, will be uninviting to most political scientists.

Yet Powell's game-theoretic study deserves close attention. The book offers an imaginative and insightful examination of a critical issue in the nuclear era—whether it is possible for a state to protect its national interest by means of a threat to use nuclear weapons against an opponent with a capability to retaliate in kind.

Two well-known theoretical attempts to deal with this problem are subjected to game-theoretic analysis. The first is Schelling's idea of the threat-that-leaves-something-to-chance, and the second is the strategy of limited retaliation. In both cases, the effects on crisis dynamics and crisis stability are assessed.

Powell models these approaches to the credibility problem by means of games of incomplete information, which are solved for sequential equilibria. The process that underlies these models is the dynamic (Bayesian)

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updating of prior probabilities by players who observe each other's moves as the crisis unfolds. This feature allows Powell to trace the net effect of complex interactions between players' beliefs and strategies. It also explains most of the results that the author presents as counterintuitive or unconventional.

For example, whereas brinkmanship crises are often said to be competitions in risk taking that favor the actor with the greater resolve, the game-theoretic model of brinkmanship indicates that such a description can be misleading. First of all, "sometimes bluffing works, and the state with the least resolve prevails" (p. 60). In addition, a state may be more, rather than less, likely to escalate when the opponent's resolve is increased.

The first result, however, does not undercut the "competition in risk taking" image. It merely reminds us that resolve has a perceptual dimension and that what an irresolute state must do in order to deceive may turn out to be quite resolute. The second result (that a greater stake in the status quo may not make crisis initiation less likely) obtains if the actors engage in a long and complicated chain of reasoning—which will be greeted with skepticism by empirically oriented readers.

The brinkmanship model also shows that contrary to intuition, the misperception of an opponent's resolve (or type) may increase crisis stability. Powell might be reminded that Stein ("When Misperception Matters," *World Politics* [1982]) demonstrated by means of a simple game model the sometimes benign effects of misperceiving a conditional strategy as a dominant one.

The findings are more surprising when the brinkmanship model is analyzed under the assumption that striking first is advantageous. The conventional logic predicts a destabilizing increase in the temptation to preempt. But the game turns out to be entirely stable: in equilibrium, neither the challenger nor the defender attacks. The reason, as Powell aptly demonstrates, is that the conventional logic treats beliefs as exogenous to crisis bargaining. Yet when beliefs and strategies are interdependent, "neither state can become sufficiently confident that war is inevitable to justify its attacking" (p. 122).

As to the debate between mutually assured destruction and counterforce, the model of limited retaliation suggests that states will in fact prefer to have more discriminatory options, as well as counterforce capabilities. The reason, however, is not damage limitation but coercive bargaining: the reduction of the opponent's options makes the opponent less likely to initiate or escalate further.

These conclusions and others that support current strategic theory rest on simplifying assumptions that detract from their persuasiveness. For example, the play of the game does not affect the players' resolve, which is assumed to be fixed; the assumption of common knowledge removes a critical element of the uncertainty that defines crises; and the exclusion of nonescalation or deescalation from the set of alternatives precludes compromise outcomes.

Unfortunately, these and other simplifications yield models that only a few would call simple. It is impossible not to wonder whether similar results could not be achieved by more tractable models.

The *descriptive*, rather than prescriptive, terms in which the results of the book are couched also invite reflection on the empirical accuracy of predictions about actors capable of the most complex chains of reasoning. Indeed, this book is symptomatic of the gulf that separates game-theoretic studies of crisis from their psychological counterparts. The cognitive effects of crisis-induced stress (even the noncrisis heuristics and biases described by Kahneman and Tversky) have yet to inform the game-theoretic literature.

Having expressed these reservations, there is much to admire in Powell's book when it is taken on its own terms. It is a study that will set a new standard for modelers, who will appreciate the ingenious formalization of theoretical ideas, the technical mastery, and the careful interpretation of the game solutions.

University of Haifa BEN D. MOR

Power in Contemporary International Politics. By Michael P. Sullivan. Columbia: University of South Carolina Press, 1990. 259p. $32.95.

This book presents a valuable summary, review, and interpretation of recent studies of international relations that impinge directly or indirectly on power issues. Michael Sullivan begins by juxtaposing the realist school of international politics with the transnational—the former operating principally from the perspective of power politics, with the latter originating largely from the principles of political economy. Along the way, he finds (not unexpectedly) that the two perspectives have a number of things in common—not the least of which is an underlying presence of power issues. And that even when the language is couched in other terminology such as interdependence or micropolitics, there is an abiding, if thinly concealed, concern with power politics.

In addition to conclusions such as these (that in themselves constitute a valuable contribution to the ongoing debates in the field of international relations), there are other positive features of the book. These are the individual discussions of major issues of concern to the discipline. Notable among them are the treatments of international polarity, parity versus preponderance, and the measurement of national power.

As we enter the post–Cold War period with its incipient multipolarity, the polarity issue has understandably emerged as a hot topic. The discussion of polarity here highlights the central question, namely, that of the superior stability of bipolarity or multipolarity and the evolution of the international system away from the confrontational bipolarity of the Cold War era. The discussion emphasizes this area as one in which cumulation of knowledge indeed has occurred, in both the clarity with which research questions have been framed and the various answers given.

The problem of polarity versus preponderance also benefits from the treatment here. As the ongoing debate in the literature reveals, the problem admits of no easy answer. Nevertheless, Sullivan's treatment summarizes not only the findings but some of the trends in East–West conflict behavior that bear on the parity-versus-preponderance issue, namely, the increase in tension when parity between East and West was more closely approximated. Indeed, much of the evidence in this book is presented in time series form either as given by the primary researchers or in Sullivan's adaptations. And this is one of the virtues of this volume, that data are presented in ways that clearly reveal trends, where they exist.

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Power (that ever elusive concept) is shown to be substantially, although not entirely, susceptible to accurate measurement. Although the measures are crude, they nevertheless have a rough face validity that enhances their usefulness. It is also one of the positive features of this volume that measures of power are compared and are frequently found to arrive at the same answer in rankings of national power.

If all this be true and trends in power, polarity, and conflict behavior were clearly discernible during the Cold War period, why was not the demise of the Cold War predicted by practitioners using these measures? After all, was it not the dismal economic situation in the Soviet Union and Eastern Europe that hastened the demise of communism? The transnationalists then would appear to have the last word. Yet if we examine the measures carefully (as Sullivan does), we see that measures of bipolarity were constantly declining, especially during the 1970s. Even the 1980s did not witness any precipitous increase. This trend is supported by indicators of détente (treaties and agreements between the United States and the Soviet Union) that spiraled upward after the late 1960s and did not appreciably diminish during the 1980s. In others words, these time series clearly indicated the ending of the Cold War that then *allowed* the demise of communism as a form of garrison state economics that simply was no longer required. With the diminution of external threat, the state-run economies of heavy industry in Eastern Europe required to build the heavy weapons of war were no longer necessary. It was against this backdrop that East European countries could be allowed to go their own way and new forms of economic systems could be attempted in these countries and now in the Soviet Union itself. It is a major benefit of Sullivan's work that this collection of time series can yield conclusions such as this. If proponents of this mode of inquiry had taken their own work more seriously, then the ending of the Cold War—if not the actual death of communism—might have been predicted with a fair degree of accuracy.

This book is an ideal choice for graduate classes and even advanced undergraduate courses in international relations research. The literature review is fairly complete and the necessity, as well as the benefits, of precision in such research are made abundantly clear.

Rutgers University, New Brunswick MANUS I. MIDLARKSY

Immigration Reconsidered: History, Sociology, and Politics. Edited by Virginia Yans-McLaughlin. New York: Oxford University Press, 1991. 342p. $39.95 cloth, $14.95 paper.

Refugees without Refuge: Formation and Failed Implementation of U.S. Political Asylum Policy in the 1980s. By Barbara M. Yarnold. Lanham, MD: University Press of America, 1990. 270p. $34.50 cloth, $18.75 paper.

Undocumented Migration to the United States: IRCA and the Experience of the 1980s. Edited by Frank D. Bean, Barry Edmonston, and Jeffrey S. Passel. Washington: Urban Institute, 1990. 292p. $38.75 cloth, $21.75 paper.

After profoundly influencing the character of U.S. society through the early twentieth century, only to be greatly restricted during its middle decades, immigration emerged again in the 1980s as a prominent and emotionally charged public policy issue. Not only was illegal immigration increasingly viewed as a critical problem for the weakened U.S. economy and sovereign integrity, but its link to hotly debated foreign policy goals in Central America broadened the interests drawn into the debate over immigration reform legislation. The 1980s also witnessed the centennial celebration of the Statue of Liberty with all the powerful meaning that symbol holds for our national identity and heritage. These three books examine in a complementary fashion important aspects of the contemporary United States' complicated immigration picture.

The volume edited by Yans-McLaughlin reassesses the historical impact of immigration on U.S. society, questioning the traditional assimilationist interpretation of the United States as a melting pot where immigrants as individuals adopt mainstream cultural and economic traits. Instead, a prominent theme running through the chapters is that structural economic and social mechanisms shape the migratory and adaptation processes. As a result, different groups of immigrants meld into U.S. society and succeed economically to a greater or lesser degree depending on the features and strength of the group networks available to aid their resettlement and employment search.

The factors behind the disparate experiences of various immigrant groups assume greater relevance today as the United States grapples with increased immigration pressures and persistent racial–ethnic tensions. Unfortunately, the volume fails to include any study on recent immigrant groups where the theory regarding the importance of structural constraints and networks is applied to the situation of Indochinese or Central American immigrants. Alejandro Portes's chapter on Hispanics discusses the different economic experiences of Mexicans and Cubans; but none attempts to apply the knowledge gained through historical analysis of immigration to increase understanding of the newest U.S. immigrants.

Other features of the book deserve mention as useful additions to the traditional study of immigration. Apart from the emphasis on structural constraints and networks, the book strives to place the U.S. immigration experience in a broader world context, where it appears less unique than is commonly touted in the traditional immigration literature. For example, Philip D. Curtin's contribution illustrates that the immigration to the United States of African slaves (forced in this case), Europeans, and Asians represented only a fraction of the overall movement of people in the tropical world during the fifteenth to nineteenth centuries. Other contributors examine the economic and political situations in sending countries, thus demonstrating the complex motivations for migration beyond the lure of the United States.

The volume further broadens the study of immigration to include African-Americans. Suzanne W. Model's piece presents an intriguing, if tentative, assessment of the less effective adaptation process of black migrants to northern urban centers as compared to South and East European immigrants. The resultant loss of employment and collapse of the nuclear family continue as two major problems facing African-Americans today.

Overall, *Immigration Reconsidered* represents a valuable contribution to immigration studies reflective of both

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contemporary scholarly concerns and U.S. immigration heritage. The other two books, however, delve more directly into contemporary immigration policy issues—the application of the 1980 Refugee Act and the effectiveness and impact of the Immigration Reform and Control Act.

Yarnold's study of the formulation of the Refugee Act of 1980 and its subsequent implementation certainly lacks the engaging narrative of the Yans-McLaughlin volume and, in fact, is almost devoid of the scene setting essential for readers with limited knowledge of the Central American migration experience. The book, instead, concentrates on investigating the variables influencing the passage of the act and its implementation—particularly as it affects asylum applicants, most of whom are from Central America. The array of data compiled and analyzed in the study strongly supports the assertion that U.S. asylum policy remains politically biased despite the intentions of the framers of the act.

Using government data on approval rates for asylum applicants, Yarnold illustrates the persistent bias in favor of persons from countries with governments "hostile" to the United States. She shows that this is the case throughout the application process so long as it remains in the hands of executive branch administrators—whether in the Immigration and Naturalization Service or the Board of Immigration Appeals. Only when cases reach the federal courts on appeal does this bias disappear; and then, Yarnold finds, other "political biases" influence the decisions. For example, judges appointed by Democratic presidents tended to rule more often in favor of asylum applicants regardless of their country of origin.

Yarnold also investigates the role of public interest organizations in the passage and implementation of the act. She examines various aspects of these types of organizations, including their ability to overcome the free-rider problem, their role in bolstering social movements, and their importance in facilitating successful asylum applications. Generally, this part of the study has less to offer than the data analysis concerning the implementation of the Refugee Act. Those results provide valuable insights into the workings of the asylum bureaucracy and the federal courts. They illustrate the ability of administrators to shape policy independently of other actors. The results also arm those critical of the political bias in the U.S. asylum policy with additional evidence.

In contrast to Yarnold's book, the volume edited by Bean, Edmonston, and Passel includes an introductory background discussion of different types of illegal immigrants and the difficulties in determining their numbers. This discussion is useful to anyone seeking a better understanding of the illegal immigration issue.

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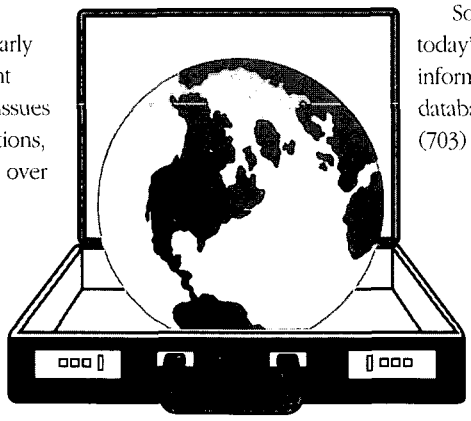

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The introduction identifies the various types of illegal immigrants, differentiating those who intend to stay (settlers) from those who come to work with the intention of returning (sojourners) and those who cross on a daily basis to work (commuters). These distinctions become crucial when trying to calculate the number of illegal immigrants entering the country (the flow) and those residing in the country at any one time (the stock). The editors point out that the failure to think of the flow, as distinct from the stock, of illegal immigrants wreaked havoc on the policy debates of the 1970s and early 1980s.

After clarifying these conceptions of illegal immigrants, the remainder of the book attempts to gauge the effectiveness of the Immigration Reform and Control Act of 1986. The act had the two goals of decreasing the stock of illegal immigrants through an amnesty provision and curbing the entry of additional illegal immigrants through employer sanctions and increased enforcement. Five of the studies seek to measure either the change in the stock of illegal immigrants or the flow of additional entrants after the passage of the act. Two studies try to ascertain changes in the perceptions and intentions of potential migrants still in Mexico. The collection represents both a timely public policy study and a challenging endeavor into a problematic area of research and data analysis. The book is a must for those involved in demographic studies calculating the size of illegal immigrant populations.

Virginia Commonwealth University- SUZANNE L. FIEDERLEIN

Political Science: Looking to the Future

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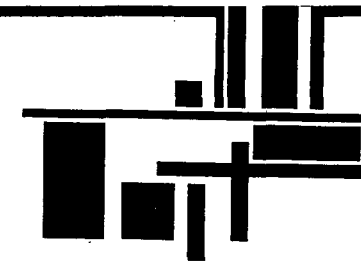

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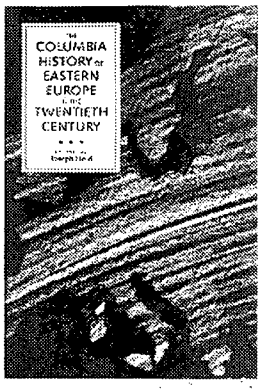

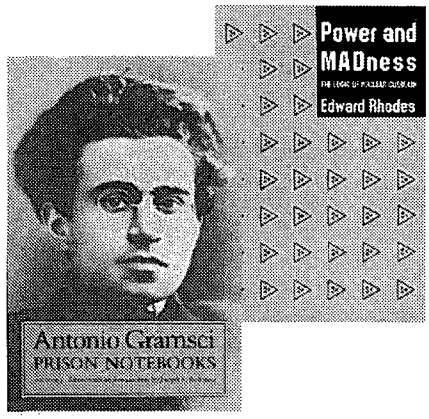

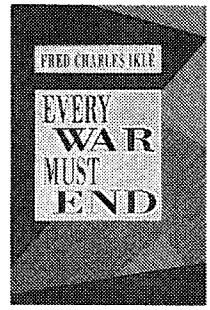

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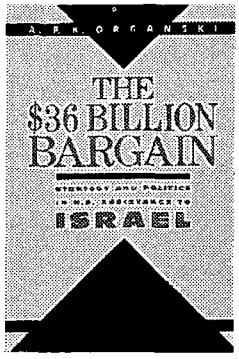

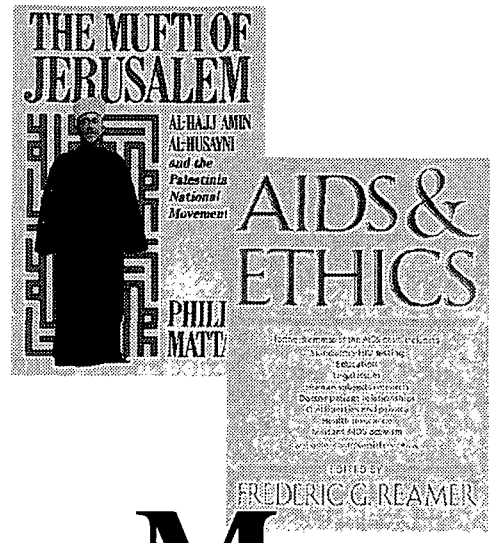

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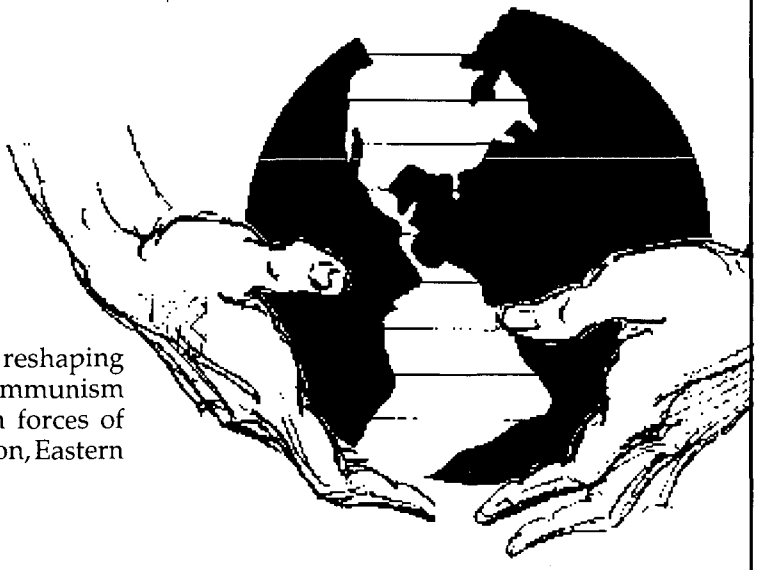

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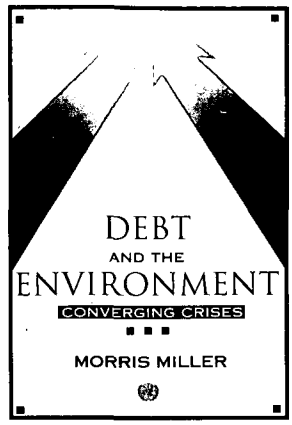

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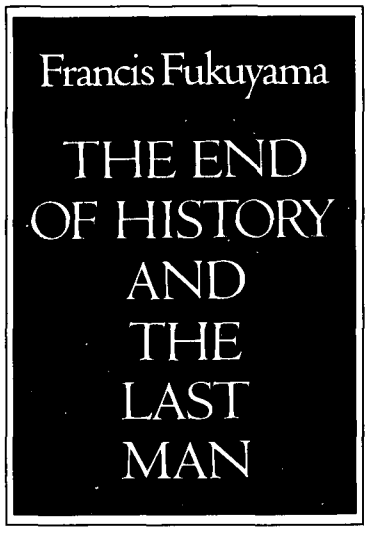

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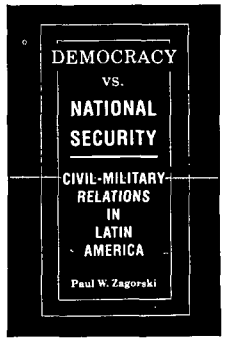

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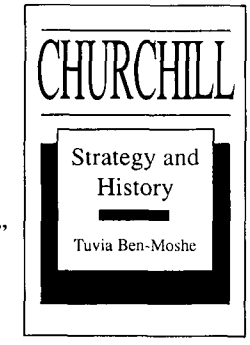

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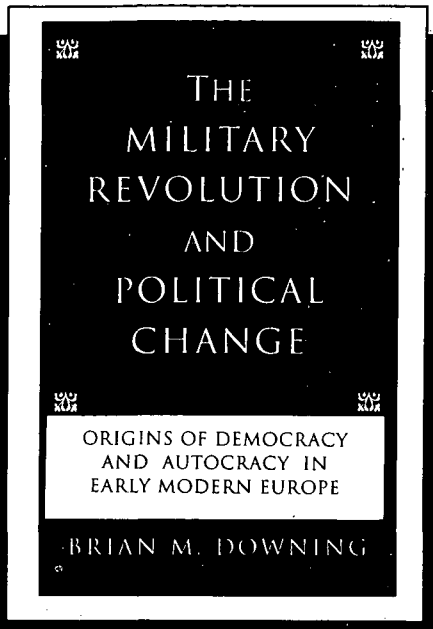

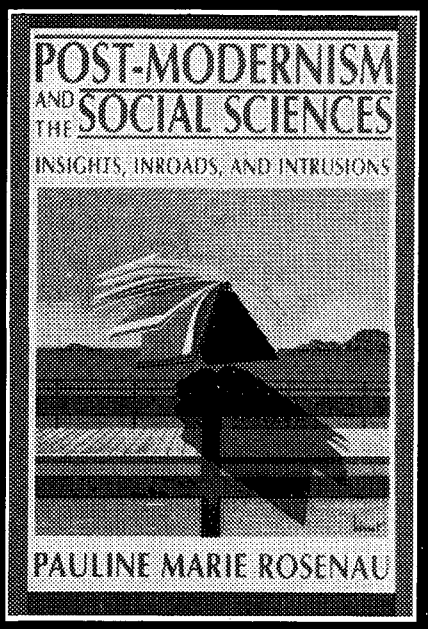

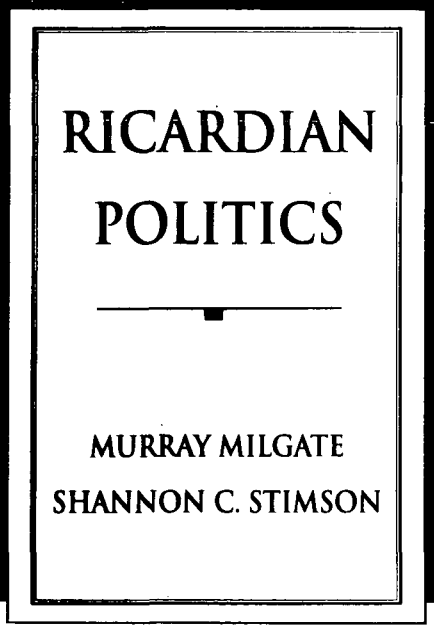

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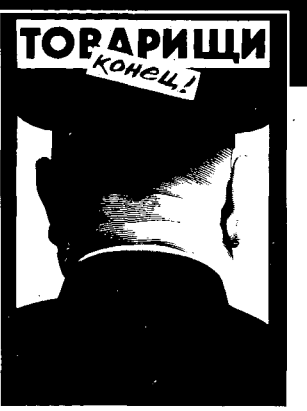

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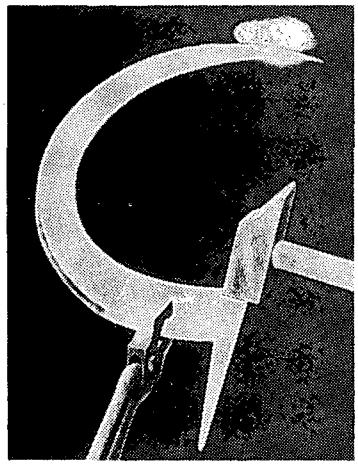

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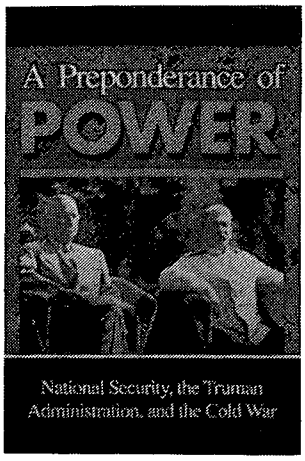

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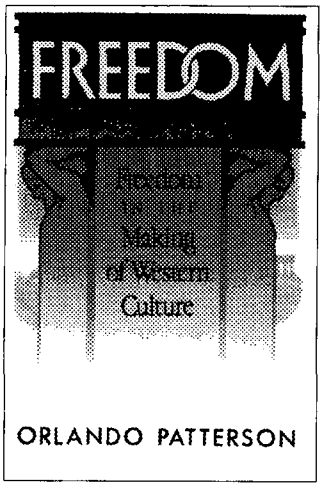

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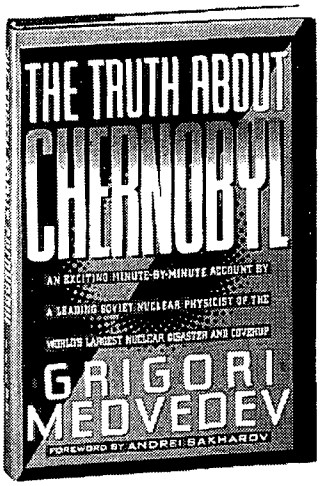

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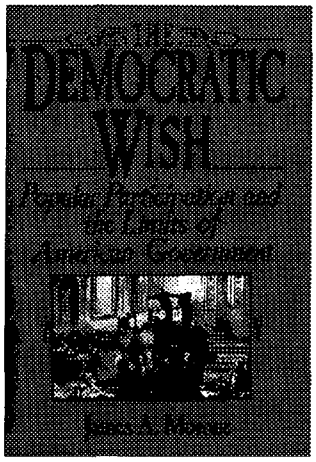

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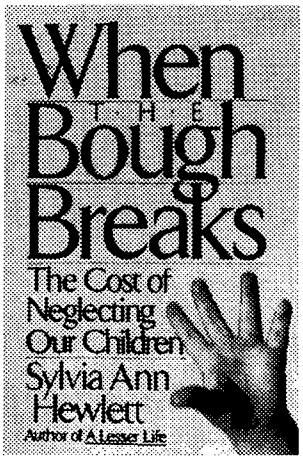

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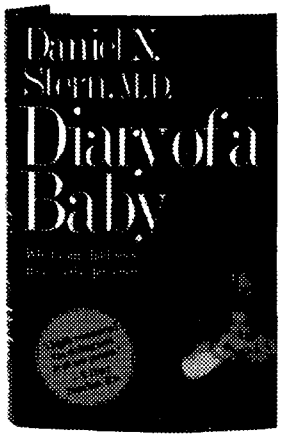

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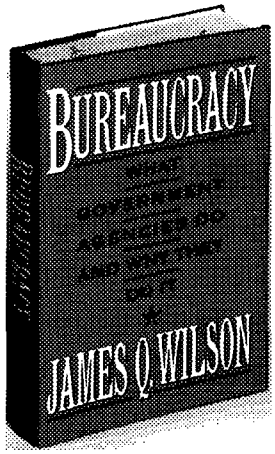

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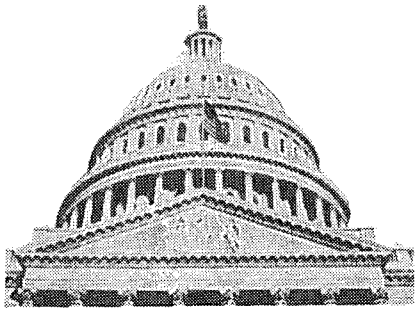

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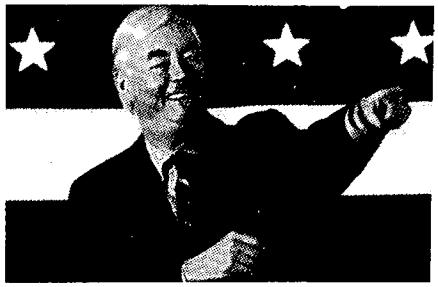

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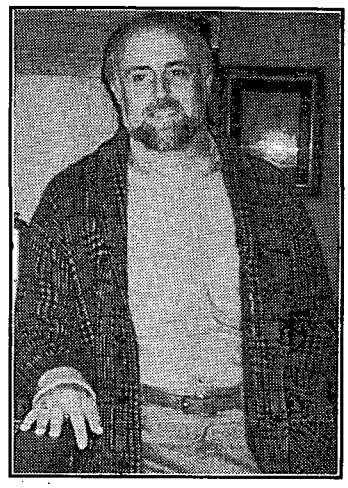

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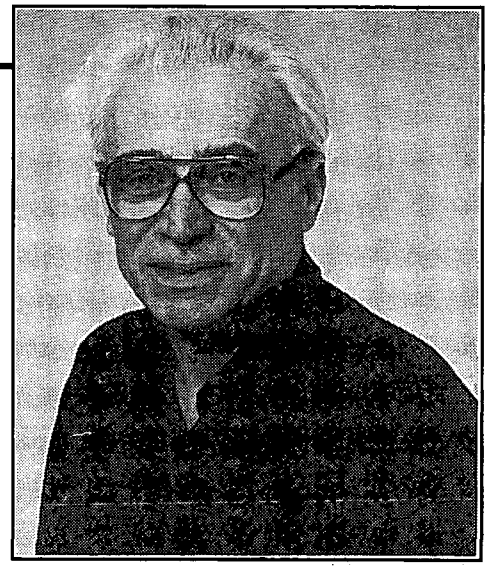

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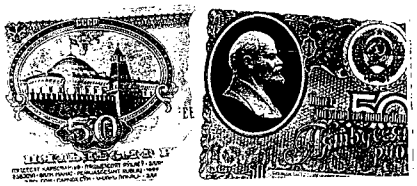

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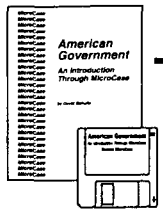

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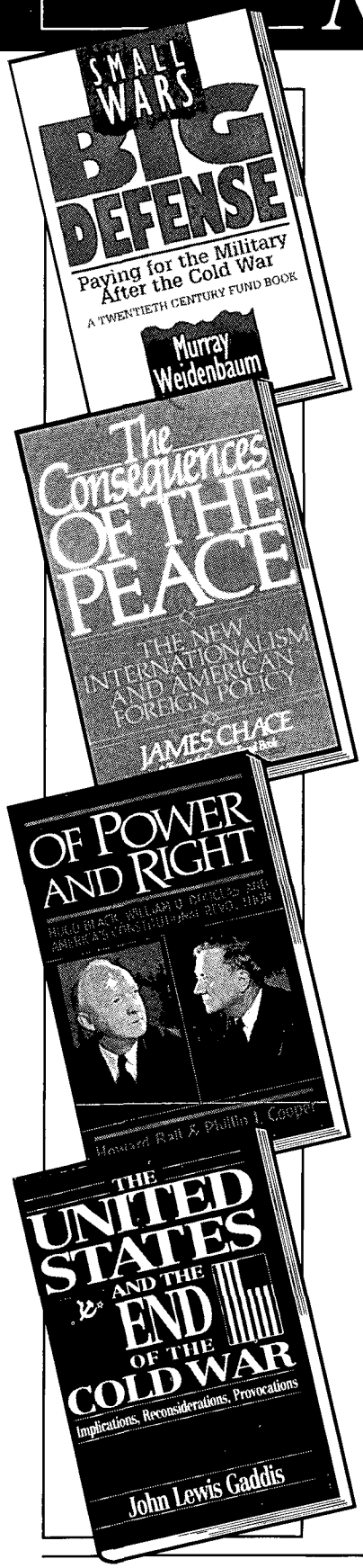

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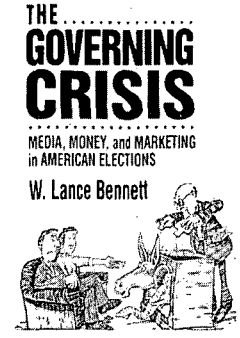

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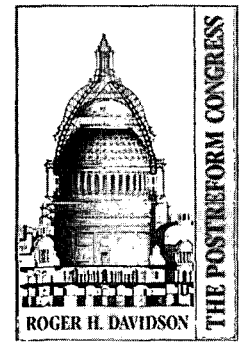

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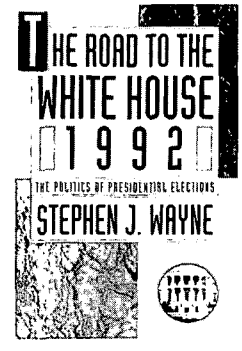

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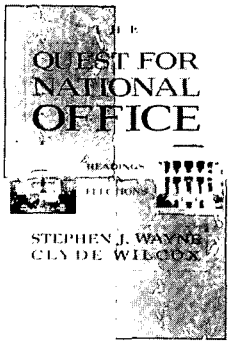

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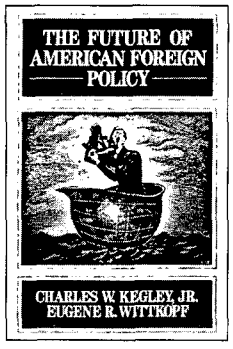

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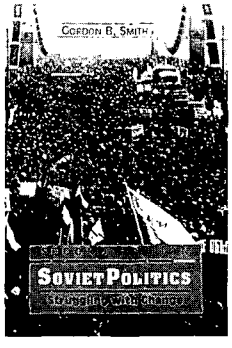

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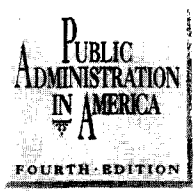

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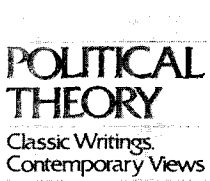

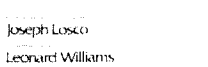

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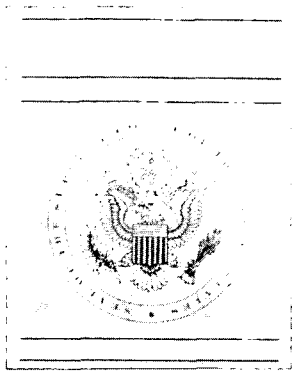

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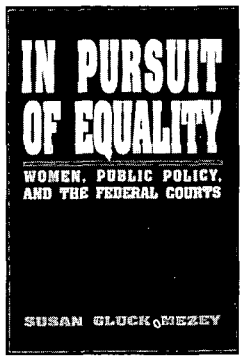

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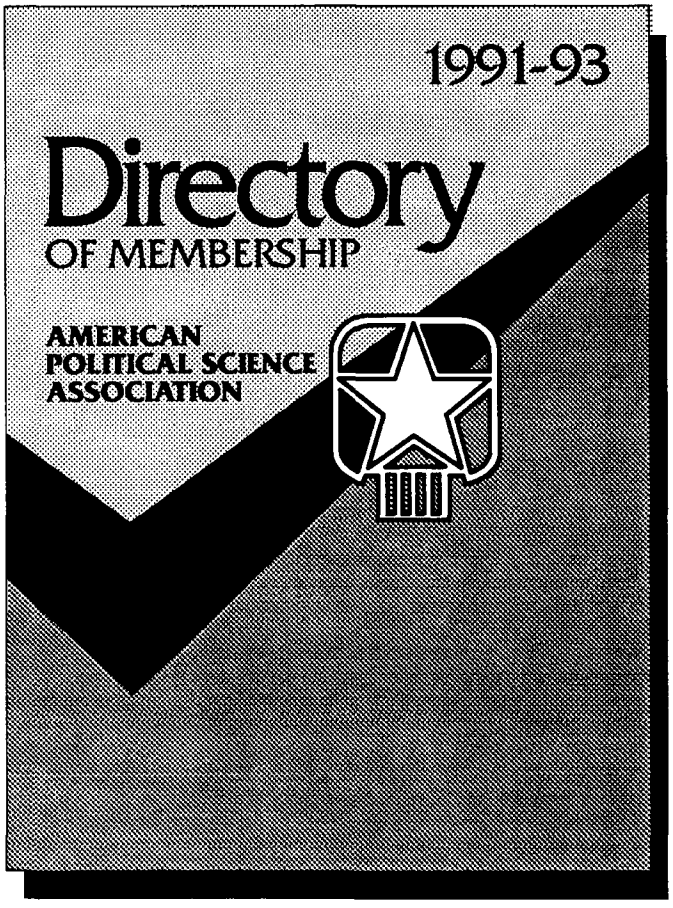

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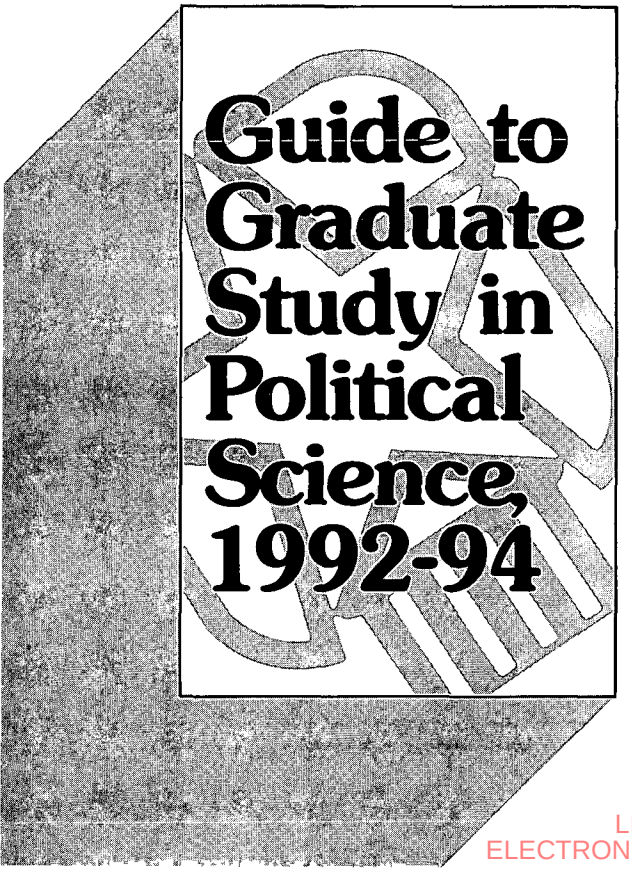

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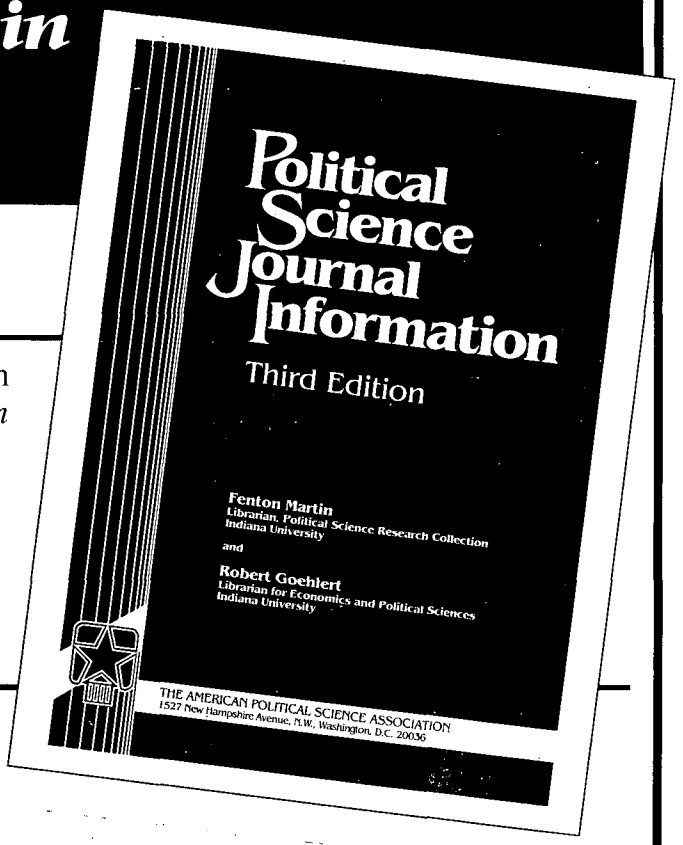

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